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SCIENCE LECTURES AT SOUTH KENSINGTON.

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AT

SOUTH KENSINGTON.

BY

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POLARISED LIGHT.

BY W. SPOTTISWOODE, F.R.S.

ACCORDING to the proposal of the director who has organised the present course, the lecture of this morning will be devoted to a description of some of the principal instruments used for investigating polarised light, and to an explanation of their use. With this purpose in view I have selected a few from the magnificent collection now in the Loan Exhibition. These you may now see on the table, and although they are only a few out of many, I doubt not that we have enough, or even more than enough, to occupy our attention during the hour at our disposal. With one exception my selection has been confined to the smaller and simpler instruments, partly because, following the example of Professor Stokes, it will be my object to explain principles rather than to repeat experiments, and to indicate what may be done with instruments of the simplest form, and even of home construction; and partly because it has been suggested that at one of the evening lectures I should make use of the larger and more elaborate instruments, and by their aid exhibit the magnificent phenomena to which the subject gives rise. And let me here take the opportunity of drawing your attention to the fact that at present the collection of apparatus appertaining to polarised light is unique, and that it furnishes the means of exhibiting the effects in question on a scale never before attempted.

Light is said to be polarised when it exhibits certain peculiarities, to be hereafter described, which it is not found

generally to possess. An instance of this is furnished by the familiar process of reflexion. When a ray of light falls upon a smooth or polished surface it is usually reflected, whatever be the angle of incidence upon the surface. And the same is true, even if the ray should undergo reflexion at a second, or a third, or at any number of surfaces. There are, however, particular substances which, although presenting polished surfaces, do at certain angles of incidence fail to give rise to a reflected ray, when after one reflexion the ray has fallen upon a second surface. Again, there are transparent substances which at certain angles of incidence fail to transmit a ray of light which has previously undergone reflexion or refraction under suitable conditions. Again, there are dispositions of perfectly colourless transparent crystals through which, if ordinary light be transmitted, splendid effects of colour are produced, and this in a way quite distinct from the prismatic dispersion explained yesterday by Professor Stokes. But in all cases where, contrary to expectation, either the reflected or the refracted ray is absent, or where colour is so produced, it will be noticed that the peculiarity occurs, not at first a reflexion or refraction, nor when a single piece of crystal is used, but when the light, having already undergone one of these processes, falls upon a second reflector or refracting substance. The light must in fact have first undergone some modification, and the effect of the modification is brought to evidence at a second process. This modification, which takes place in various forms, and which may be effected in various ways, is called Polarisation; and the process by which light is examined, whether it be polarised or not, is called Analysation. Similarly, the instrument, of whatever kind, used for the first process, is called the Polariser; and that used for the second the Analyser.

Let us now come to closer quarters with the subject. If I take a flat plate of ordinary glass and interpose it in the path of a beam of light, part of that light will be reflected from the surface, and part will be transmitted through the plate. And this will be the case at whatever angle the light falls upon the glass. And if either the reflected or the refracted beam be received upon a second plate, the same results will in general ensue. If, however, the beam of light being in a definite direction, say horizontal, the first plate of glass be inclined at about one third of a right angle

to the beam, then both the reflected and the transmitted rays will be found to be in peculiar conditions. For if either of them be received upon a second plate of glass at a similar angle of incidence, then it will be found that in certain positions of the latter, or analyser, as I may at once call it, the reflected ray, and in others the transmitted ray, will be wanting. To explain these positions I would remind you that while the glass plates maintain the same angle of incidence towards the ray incident upon each, they may each be turned into various positions. Thus either of them may be so directed as to face upwards, or downwards, or towards you, or towards me. Suppose, for example, that the first plate, or polariser, faces upwards, and that we consider only the beam, which having been reflected from the polariser is either reflected from, or transmitted through, the analysing plate; then it will be seen that when the analyser faces either towards you or towards me, the reflected rays will be wanting, but the transmitted rays will be present. When, on the other hand, the analyser faces upwards or downwards, the transmitted rays will be wanting but the reflected present. And further, as the analyser is turned round from one of these positions to the other about the beam as an axis to which it always remains inclined at the same angle, the relative brightness of the two beams will gradually alter. When the angle of turning amounts to 45°, one will have increased and the other diminished in brightness, so that both are of the same intensity; and when the angle reaches 90°, the beam which had been first extinguished will have attained its full brightness, while that which had been bright will have been extinguished. When these peculiarities are found to appertain to either the reflected or the transmitted beam, the light so examined is said to be polarised. And it is to be observed, first, that the direction in which this peculiarity is found depends upon the position of the first glass plate, or polariser. For instance, when the polariser faces upwards the beam which would be reflected from the analyser towards you is found to be extinguished; then, if the polariser be turned so as to face towards you, the ray which would be reflected from the analyser upwards will be extinguished. And so likewise for any intermediate position of the polariser. Also, since the effect in question appertains to the entire beam of light, it appertains to every

ray of which it is composed ; in other words, the effect belongs to a general direction running through the whole, or to a direction determined by a plane, *e.g.*, that of the polariser. Moreover, since, as described before, the beams reflected and refracted by the analyser are respectively extinguished by the analyser in two positions at right angles to one another, the two beams are said to be polarised in two planes at right angles, or to have their planes of polarisation at right angles to one another. This, then, is one test of polarised light. If we proceed in the same manner to examine the beam transmitted by the polariser, similar effects will be noticed, except that the positions for extinction will be reversed. The extinction described is, however, in fact, only partial, not complete ; but it will be enhanced or rendered more complete if, instead of using a single plate of glass, we use a number of plates placed close together. The reason is this, that the effect is produced in the passage of the light through the glass, and the effect produced by one plate is carried on to a greater degree by a second, and so on until by the use of a sufficient number a state of polarisation as nearly complete as we desire may be obtained. With a dozen plates of thin microscopic glass excellent effects are produced ; but five or six plates of ordinary thin glass, say $1\frac{1}{2}$ inch long and $\frac{3}{4}$ inch broad, held obliquely at the angle before described, will do very well, and is altogether preferable to a single plate. From the fact that, when the plates are properly placed, both the reflected and the transmitted rays are polarised, there follows the important principle that, whether we use reflected or transmitted light, we may at pleasure interchange the polariser and analyser ; in short, that any instrument which will serve for one purpose will serve for the other. The bundle of glass will therefore furnish you with a beam of reflected and a beam of transmitted light, both polarised ; but since the transmitted beam preserves its direction when the bundle is turned round, while the reflected beam does not, it is generally more convenient to use the former.

It was on account of the fact that a beam of light, when acted upon in a certain direction across its line of progress, is under certain conditions capable of being extinguished by processes which do not ordinarily extinguish light, that a beam of light when so acted on was said by Sir Isaac Newton to have sides. And inasmuch as this effect had reference to direction, it was

supposed to have some analogy to the effects of magnetism or electricity, or to other actions comprised under the general term polar. And light so affected was therefore said to be polarised. The term is perhaps not altogether felicitous or self-explanatory, but it is probably too late in the world's history to change it.

And now, passing from the direct instrumental examination of polarised light, it is necessary to say a few words on the physical explanation which has been proposed of the phenomena. Light, according to the wave theory, is supposed to be due to the vibrations of a certain elastic medium which is diffused through all space, and which is capable of undergoing vibrations. The vibrations, to the effect of which on the retina the impression of light is attributed, are extremely small in amplitude and extremely rapid in succession; and, for reasons too long to be enumerated here, may be regarded as always taking place in planes perpendicular to the ray. In these planes the actual motion may have any direction and form compatible with the mechanical constitution of the vibrating medium, or ether as it is called. But, in particular, there are reasons for thinking that the paths or orbits are always straight lines, or circles, or ellipses. The straight lines, and the longer axes of the ellipses, may have any direction in the perpendicular plane through the ray; and the direction of motion in the circles and ellipses may be either direct or retrograde.

We will first consider more in detail the case of rectilinear, or straight line, vibrations. The general principles which regulate this case are applicable, cæteris paribus, to the others; but it is convenient to begin with the simplest form. Imagine then a row of particles of this ether lying originally in a straight line to be successively disturbed from their positions of rest, and then allowed to fall back again. The successive motion of the particles may be due to their mutual attractions when one of them has been once disturbed; or it may be due to other forms of elasticity. But whatever be the physical cause, the motion gives rise to a wave-like configuration which is passed on throughout the row of particles, and produces what is understood as the progressive motion of the waves. Now in ordinary, or unpolarised light, the successive disturbances or impulses communicated to any particle need not be all in the same direction; and there is experimental

evidence for thinking that at different parts of the ray the directions undergo abrupt changes. In polarised light, on the contrary, whatever be the direction of vibration in one part, the same is maintained throughout the whole ray. That is the mechanical explanation of polarisation. If then the transmission of a beam of light through a bundle of glass plates such as I have described has the effect of bringing all the vibrations into a certain direction dependent upon the direction in which the plates face, it is not difficult to conclude that the effect of a second bundle upon the polarisation of the rays will be nil if it faces the same way as the first. To fix our ideas, suppose that the vibrations of the rays reflected from the first bundle be horizontal; or in other words, that its facing (or azimuth as it is termed) is such that it will reflect only horizontal vibrations. Then it is clear that if the second bundle have the same azimuth it will not affect the direction or plane of polarisation, because it has received those vibrations, and those only, which it is capable of reflecting. If, however, the second bundle be turned in the manner described before, the direction of vibrations which it is capable of reflecting will turn with it, and the vibrations falling upon it will be oblique to those which it can reflect. Now motion oblique to a given plane may be considered as composed of two motions partly in the plane, and partly perpendicular to it; because if the oblique motion be replaced by these two motions executed successively in suitable times, the moving body will arrive at the same position in the same time, whether it has passed by one of these paths or by the other. The oblique motion is then said to have been resolved in two directions, viz. parallel and perpendicular to the plane in question. The second bundle of glass will therefore reflect only that part of the vibrations falling on it which can be resolved into its own plane; the part resolved into the perpendicular direction will altogether fail to be reflected. The amount of light reflected will consequently diminish according as the second bundle has its azimuth turned away from that of the first; and when the turning has reached a right angle, no light at all will be reflected.

Now for producing these effects we are not confined to glass; but we may use instead almost any other substance, except metals; and the only question is, at what angle incidence must take place in order most effectually to polarise the

light. In the case of transparent substances there is a very simple law, as follows: When light falls upon the surface of a transparent medium part is in general reflected and part, after undergoing refraction or bending, is transmitted. The angle of reflexion is, as you well know, equal to that of incidence; that of refraction depends upon the relative densities of air and of the transparent medium; and the angle between the reflected and the refracted rays will therefore vary with the angle of incidence, and when the latter angle is such that the reflected and refracted rays are at right angles to one another, the light, both reflected and refracted, is most completely polarised. This angle of incidence, which is of course different for different substances, is on this account called the polarising angle. You will doubtless remember that the polarisation of the reflected ray is perpendicular to that of the refracted ray.

Beside the method which we have as yet used, viz. reflexion and refraction by glass, there are other methods of polarising and analysing light. The most important of these, and the one which I now propose for your attention, is the transmission through crystals. Generally speaking crystalline bodies reflect and refract light like glass, or water. But, unlike glass or water, they divide every ray which they transmit into two. Here is a block of Iceland spar, or calcite, the crystal generally used in researches on polarised light; first, because it is perfectly colourless and transparent; secondly, because it is found in masses sufficiently large for all optical purposes; and thirdly, because it possesses the power of separating the rays into two in a very high degree. Its natural form is that of a rhombohedron, having two obtuse and two acute angles. Rays of light passing through such a crystal are, as observed before, generally divided into two, or *doubly refracted.* But there is one direction, and one only, viz. that of a line joining the two obtuse angles, in which no such division takes place. This line is called the axis of the crystal. The greater the angle made by the incident ray with the axis, the greater the separation; and at right angles to the axis the separation is greatest. The block of spar which I hold in my hand has had its two blunt angles cut off; and if we look through the faces so artificially formed, we shall see a single image of objects beyond; but if we look through in any other direction we shall see two images.

We are not however here so much specially concerned with the fact of double refraction as with a peculiarity which accompanies it, viz. that each of the rays produced by double refraction is itself polarised, and that the two are polarised in planes at right angles to one another. This is seen by analysing two such beams of light. If the analyser be placed so as to extinguish one of them, the other is at its brightest. If the analyser be then turned round the first beam will begin to appear, while the second fades. When the angle of turning reaches 45°, the two are equally bright; and when it reaches 90°, that which has been extinguished is at its brightest, while the other has disappeared. An arrangement of spar producing these effects with the greatest degree of separation between the two sets of rays, is called a *double-image* prism; and from its convenience in turning round is in many ways preferable to the glass plates. But when great size is required, it is both difficult and expensive to obtain suitable spar for the construction of the instrument.

It is, however, not always necessary, nor indeed always convenient, to have in the field of view both the images due to double refraction. We often require a simple beam of polarised light, and in such case a double image prism gives us more than we want. To meet this inconvenience, a very ingenious arrangement was devised for getting rid of one of the beams. A block of spar so prepared as to give good double refraction is cut diagonally across, and cemented together again with Canada balsam. The refractive power of this substance is different from that of the spar: and the angle of cutting is so arranged that at the surface of the balsam one of the rays undergoes total reflexion, and is thus thrown entirely out of the field. There then remains visible only a single beam of perfectly polarised light. This instrument is named after its inventor, a Nicol prism. The end faces of such a prism are very delicate, and should be preserved and cleaned, when necessary, with the greatest care. A pair of instruments of whatever kind, one of which is used for polarising, the other for analysing, light, is called a *polariscope;* and a pair of Nicol prisms, or a Nicol and a double image prism. is the most convenient form of polariscope as yet invented. It is not, however, essential that the polariser and analyser should be instruments of the same kind; we may polarise by reflexion or refraction with glass plates, and

analyse by a Nicol prism, or *vice versâ*. In fact, any instrument which will serve as a polariser will serve equally well as an analyser. And by means of any one of these instruments we can convert a beam of ordinary into a beam of polarised light, whose plane of polarisation lies in any required direction.

Now out of this effect of double refraction another important property of crystals follows as a consequence. When a ray of light passing from a rarer to a denser medium, as from air to glass or water, falls obliquely on the surface separating the media, it is refracted or bent out of its original direction towards (*i.e.* in such a way as to make a smaller angle than before with) the perpendicular drawn at the point of incidence to the surface. This refraction or deviation is due to the slower rate at which the light travels in the denser than in the rarer medium. Hence we may conclude that of the two rays produced by double refraction that which is most deviated moves slowest. The entire action of the crystal is consequently, first, to divide each ray into two, or doubly refract it; secondly, to polarise both these rays in planes at right angles to one another; and thirdly, to make one of the rays to traverse the crystal slower than the other, or to *retard* one more than the other. If, therefore, we consider, as we must, that the two sets of rays consist of waves of the same length, then the waves of the one set will be retarded, or made to lag behind the other to a certain extent; and if by some mechanical or optical arrangement we bring these two sets of vibrations, which are now at right angles to one another, into one and the same plane after they be emerged, then instead of the waves being exactly coincident as they would have been if the light had moved all with the same velocity through the crystal, the one will be somewhat behind the other; that is, the crests of one set of waves will be behind the crests of the other set, and the hollows of the one set behind the hollows of the other. Although the result of this is pretty well known, I will, nevertheless, bring to your notice a little instrument devised by the late Sir C. Wheatstone, by which the effect is very clearly shown. As now arranged, the two sets of waves coincide; but by moving a slide so as to bring the crests of one set to coincide with the hollows of the other, the effect is to reduce the combined wave to a straight line, whereas, when the crests are coincident the

compound wave will be double the height of each of the component waves. As one is gradually retarded or thrown behind the other, the compound wave becomes less and less, until at last, when the crest of one wave is opposite the hollow of the other, the combined wave is absolutely destroyed. And the same thing occurs if one wave is advanced in front of the other.

Now consider for a moment what has happened. There we have two Nicol prisms. A ray of light after passing through the first emerges polarised. Suppose for a moment that the vibrations are horizontal; this being so, if the second prism is in a similar position, the light would pass through. But if the second prism be turned round through any angle it would only transmit the oblique vibrations, partly horizontal and partly vertical, and when we turn it round at right angles to the first, the light will be extinguished. Supposing now that I introduce into this space a plate of crystal, such as mica, upon it there falls a certain beam of light whereof the vibrations are horizontal. On entering the plate every ray of the beam will be divided into two, and the vibrations in one of those rays will be in one direction, and the vibrations in the other will be at right angles to the first. Suppose, for convenience, the plate to be so placed that the vibrations which it will transmit are at 45° to the horizontal, say, north-east and south-east, and north-west and south-west, or the dexter and sinister diagonal, as it is termed. Now when the light emerges from this crystal we shall have two sets of vibrations taking place in two rectangular directions, and those forming one of those rays will be retarded behind the other. The extent of the retardation is very small in absolute amount, but, nevertheless, optically quite appreciable. Supposing then, we cause these two sets of vibrations to fall upon this second Nicol prism, or analyser; the effect of that is to resolve these two vibrations, or such parts of them as are capable of being resolved, into a single plane. They may be resolved into any direction at pleasure, by giving a suitable direction to the analyser. Suppose that they be resolved into a vertical plane, we shall have two sets of waves (portions of the two oblique waves issuing from the plate) in a vertical plane, one of which is somewhat behind the other. Now, as Professor Stokes described to you yesterday, white light is compounded of light of various kinds, *i.e.* of various wave-

lengths, or of various refrangibilities. All the waves of which that white light is compounded are retarded through the same absolute distance, the same fraction of an inch, by its passage through the crystal plate, and therefore the waves of different lengths will have been retarded through different fractions of the wave-length. In other words, the distance through which they are retarded being the same for all wave-lengths, will be a larger fraction of the short waves than of the long waves. The amount of retardation depends, as before observed, on the thickness of the crystal; and the thickness may be such as to produce a retardation which, for some particular wave, is exactly half a wave-length. If so, that particular set of waves will, as the machine showed us just now, be annihilated or extinguished. By this process, therefore, from the white light one particular colour will have been subtracted, and there will remain an assemblage of colours which being incapable of reproducing white light will combine to form a residual tint. It is of great importance to realise this process. To recapitulate, if between the polariser and analyser there be introduced such a crystal, we shall not only have the effect I have described in the first instance, of extinguishing the light, but when the analyser is in the proper position, we have colour produced by the extinction of a certain portion or portions of white light. There are different kinds of crystal which for the same thicknesses produce different effects, but for the same crystal it is easily seen that the amount of retardation will depend on the thickness of the plate, because the retarding action of the substance through which the divided rays pass, is in operation during the entire passage. If, therefore, a crystal of one thickness extinguishes one set of waves, a thicker plate will extinguish a longer set, and generally different thicknesses of any given crystal will extinguish different component portions of the white light, and the resulting colours will also be different. In fact, by carefully selecting plates of suitable thicknesses we can produce in the field of view any arrangement of colours at pleasure.

The majority of crystals polarise light in the manner above described; but there is another kind of crystal which produces polarisation of a different kind. I said, in the outset, that the vibrations might be either straight lines, or circles, or ellipses; but we have hitherto confined our attention to

rectilinear, or straight line vibrations. I now propose to say a few words upon circular and elliptic polarisation. We may suppose a ray of common light to be due to vibrations of the latter kinds; but in such a case the direction of motion as well as the position of the axes of the ellipses may be subject to abrupt changes at different parts of the ray; but when the ray is polarised and the motion in one part of the ray is in the direction of the hands of a watch, say right-handed, it will continue so throughout the entire ray. The well-known crystal quartz has the property, in a certain particular direction, of producing not only two rays as crystals generally do, but two rays in which the vibrations are circular, one of them being right-handed and the other left-handed.

We have now gone nearly as far as our time will permit, and I sincerely hope you will repeat some of these experiments for yourselves, and that with this view you may take the opportunity of seeing them exhibited here in the evening. But before concluding I would strongly advise any who feel at all interested in the subject to begin with a simple bundle of glass; the plates should be about the size of your thumb, as thin as you can get, and as numerous as you can work with conveniently. You may use from five to ten or twelve, if you can manage to get them thin and clear enough. Begin your experiments on polarisation by examining reflected light; and if you are in a room or in a garden take the reflections from the furniture, from the walls, from leaves of trees, or anything of that kind, and you will be interested to find how difficult it is to find any which bear no traces of polarisation. You may then vary your experience by taking a piece of ordinary mica, such as is used for covering gas burners, split into various degrees of thinness, the thinner the better, place it in the passage of the polarised light, and you will see most vivid gorgeous colours. If you wish to go a little further, you may have an instrument more or less like the one I have here. It consists of a Nicol prism, a double image prism, a plate of quartz, and a piece of crystal called tourmaline. The latter has the same effect of producing a single beam of polarised light and is very convenient, but it has the extreme inconvenience that it is almost impossible to get a plate without colour.

The phenomena which I have spoken of hitherto are dependent upon the properties of rays of parallel light; but

there are others which are due to convergent light. If light is made to converge on a glass or crystal and to emerge in a divergent form, a variety of effects are produced, not a few of which may be seen by using this piece of tourmaline. You will easily understand if you get a piece of crystal close to your eye you are really examining it by means of a convergent and divergent pencil. But if it is placed at the distance of ordinary vision you are using practically parallel light. If it be brought so close to your eye that you use convergent light, you will be able to observe some of the phenomena for which much more elaborate instruments are usually employed. By properly arranging your experiments you may begin with the inexpensive glass plates described before. You may then advance to apparatus a little more elaborate, but even then you may procure a piece or two at a time as you find convenient; and by degrees build up a polarising apparatus such as I have here.

I have not time to speak of the applications of polarised light, but I have here an instrument which is of commercial value, called a saccharometer, the object of which is to ascertain the amount of saccharine solution present in a fluid. It is based on the principle I last described, namely, this rotary polarisation, as it is called. Besides quartz there are many solutions, particularly those of sugar, which have the power of turning the plane of polarisation of the rays of light, and producing the effects I have described. For a given thickness or length of column the amount of this turning depends on the strength of the solution. Therefore by an accurate measurement of the amount of turning the strength of the solution may be tested.

Besides the polarisation due to the reflexion of light from furniture, books, leaves and so forth, polarised light is to be found present in a clear sky, and its nature may be tested by any of the instruments described above.

The full limits of this sky polarisation have not been completely laid down. They were studied in the first instance by Sir David Brewster, and several of his papers, although excellent, have hardly been thoroughly understood; and indeed the subject is likely soon to undergo fresh examination. Nevertheless you will find it very interesting in walking about on a clear day to examine what parts of the sky show traces of polarisation, and how the plane of

polarisation is connected with the position of the sun. But the position of the sun, or, as I ought here to express it, the position of the plane of sky polarisation, reminds me that it is time to draw this discourse to a close. I therefore bid you farewell, with a hearty wish that you may pursue this subject further for yourselves, and with a firm conviction that you will find any labour bestowed upon it abundantly repaid.

THERMAL CONDUCTIVITY.

BY PROFESSOR FORBES.

THIS morning it is my duty to explain to you as far as possible the facts that have already been learnt as to the nature of the conduction of heat, and to illustrate the progress of our knowledge in this subject by the original apparatus which has been used by different experimenters. You are all aware that the general equilibrium of the temperature is maintained in virtue of three different processes. Different bodies at different temperatures have a general tendency gradually to arrive at a certain uniform temperature, and this is arrived at by the process of conduction, by that of convection, or by that of radiation. Radiation consists in a transmission of the state which we call heat through space without affecting any material object intermediate between the warm and the cold object. That is, the sun heats the earth by radiation, and so a general equilibrium of temperature is gradually being established between the sun and the planet. Secondly, if we have a bar of any metal, and place one end of it to a fire, the other end will gradually become hot. This is quite a different means of equalising the temperature. In this case the particles of the body intermediate between the one end and the other have gradually got warmer and warmer, and thus the heat is gradually transmitted from point to point of the metal. This is the nature of true conduction. In the third place, if the hot and cold bodies be in a liquid whose parts are capable of moving about freely amongst themselves, then, by the increase of temperature which the liquid which surrounds the hot body acquires, that part of the liquid is rendered lighter, so

that it rises and gives place to a colder liquid, and consequently, in the case of a liquid in an unequally heated space, there is a continual circulation of currents going on, tending to equalise the temperature of the space inclosing the liquid. The same is true with gases.

Now, the question of conduction is a very important one, both theoretically and practically. The most simple experiments show us that different bodies vary enormously in their conducting powers. For example, a spoon placed in a cup of tea will soon become hot throughout, although there is only a small part in the hot tea. If you were to put a stick of glass instead of a metallic spoon into the tea, the other end of the glass would never increase perceptibly in temperature, thus showing that the conductivity of glass is much less than that of the metal. Experience shows invariably that metals are all better conductors than organic or other substances, and the question now arises how we can determine a method by which the conductivity of substances can actually be measured. We must define conductivity, not only in such a way that we can actually give a number to represent the conductivity of each different substance, but after we have acquired this knowledge of what is the exact conductivity of different substances, we can then apply our knowledge to different purposes, theoretically and practically. For example, there is an enormous loss of heat from the boiler of a steam-engine, a loss of heat which is produced by radiation into space. But if, as is very common now, the boiler is surrounded by a cement which is a bad conductor of heat, then there will not be such a great loss of heat, so great a quantity of heat will not be allowed to pass through this non-conductor, and consequently more is retained for the economical purposes of working the steam-engine. Various other applications will occur to you, but in the meantime we must go on to the definition which has been given of conductivity.

The first man who took up the subject of conductivity in a scientific and mathematical manner was the French physicist, Fourier. He wrote in 1812 a most valuable work entitled *The Analytical Theory of Heat*, but, owing apparently to the jealousy of other members of the Institute of France, this work was not published until twelve

years afterwards, and it was only by consulting the original MS. in the possession of the Institute that Poisson and others were able to study the results which had been arrived at by Fourier. Fourier defines conductivity in a manner which is easiest explained by means of a simple illustration. Suppose that I have a solid wall separating two chambers which are of different temperatures. Suppose that on one side of this wall the temperature is 2° higher than it is on the other, then we know that heat will pass from the hot side to the cold side. The question is, how much heat will pass? Fourier says, let us continually keep the two sides at a constant temperature, so that when heat passes out from the outside an additional amount of heat is always being added to keep the temperature constant, while at the cold side let heat be continually drawn away, let cold matter be continually added, so as to keep the temperature there constant also. Then, if we can measure the amount of heat which passes through this wall, we are able to measure exactly its conductivity. You see that, the longer the time that elapses the greater is the amount of heat which will pass through. Therefore, if we call the quantity of heat which passes through, or the flux of heat, by the letter F, the flux of heat is proportional in the first place to time, and we will call the time T. Then, again, suppose we take another wall of exactly the same substance, the temperature of the two sides of which are also separated by two degrees, the highest temperature in this case being the same as the lowest in the case we considered before. The circumstances are almost identical, the difference of temperature is the same, and the thickness of the wall is the same, consequently the same quantity of heat will pass through this wall in the course of a given interval of time. Now let us place these two walls against each other, the one overlapping the other, and let us have the temperature of the two vessels now differing by 4°, then half way through these two walls we shall have the mean temperature, and consequently each of these walls will be in identically the same condition as it was when there was only a single wall employed; the difference in temperature of the two faces of each wall will be exactly the same as it was before, and the same quantity of heat will pass through. Consequently, if we double the difference

of temperature between the two vessels and double the thickness at the same time, the same quantity of heat passes through, and that is the point which we wish to arrive at. We see, then, the thicker the wall is the greater is the resistance to the flux of heat, and the greater the difference of temperature between the two faces of the wall the greater amount of heat is allowed to pass, and consequently the flux of heat is also proportional to the difference in temperature, which I may call $\theta_1 - \theta_2$. The difference in temperature between the first and second wall is thus universally proportional to the thickness of the wall, and therefore, if the thickness of the wall be called D, in the result the quantity of heat which passes through the wall will depend on the area of the wall, consequently, if we call A the area of the wall, you see that the flux of heat will be proportional to A. Hence, altogether, the flux of heat through the wall varies directly with the time, directly with the difference in temperature between the two faces, directly as the area of the surface through which the heat is passing, and inversely as the thickness of the wall. Since F varies proportionally to this, we are justified in saying that the flux of heat is equal to this quantity multiplied by a constant, which I call K. If, then, in any experiment we are able to measure the flux of heat, if we can measure the difference of temperature in the two faces and the area through which that heat is allowed to pass, the thickness of the wall and the time during which that heat is passing through, then we know all these constant quantities except one, and we may state the result thus:— $F = K\, t \frac{\theta_1 - \theta_2}{d} A$. All we have to do, then, is to determine K in this formula, which is called the conductivity of the substance. The greater K is, the greater is the flux of heat. K is the quantity which must be determined experimentally in order that we may apply our knowledge of the conductivity of different substances to practical or theoretical purposes. We find in the end that the conductivity is equal to the flux $K = \frac{F.\, d}{t \times (\theta_1 - \theta_2)} A$. That is the value of the conductivity in any experiment that we make.

Now, to define the conductivity more simply, let us take particular values of some of these quantities. Let us make

the thickness of the wall a unit of length, and it is customary nowadays to employ generally the centimeter, the gramme, and the second as our units of length, mass, and time. Consequently, let us make our wall one centimeter thick, and then in place of "d" we should have to write "one." Let us take the difference of temperature as equal to 1°, and then instead of $\theta_1 - \theta_2$ we have to write 1°. Let us take the area of our wall through which the flux of heat is measured as equal to one centimeter squared. The unit of area A, then, we replace again by one. If we measure the quantity of heat which passes through in a minute, then $\frac{1}{60}$ will pass through in a second, and let us take one second as the unit of time instead of T. Then what we measure is the flux of heat through a piece of the substance one centimeter thick over an area one centimeter square, with a difference of temperature of 1° and during one second of time, that is, the conductivity is the quantity of heat which flows across from unit of surface of a body whose thickness is one unit of length and the difference of temperature of whose sides is a unit of temperature during unit of time. That is Fourier's definition of conductivity.

Now, there are very great practical differences in determining the value of this quantity K, but there is one condition which was pointed out by Fourier, and which has been almost invariably used for the measurement of the conductivity of different bodies. This is what Fourier called the permanent state. If I take a rod of metal, such as this, and heat it by any means at one end and let the other end gradually cool by radiation to the air and by convection, then this end will always be the hottest, but a certain amount of increase of temperature will be propagated along the bar, and finally, after some hours it may be, the rod will acquire a permanent condition of temperature, provided the temperature of the heated end remains constant and the temperature of the air remains constant. Then we shall have a perfectly gradual decrease in the temperature from the hot end to the temperature of the air at the other end, and a steady flow of heat is passing through the bar in order to maintain this constant condition of temperature. Under these circumstances, when a body is in such a permanent condition, it is possible to measure the flux of heat. Fourier gives a number of formulæ which were

applicable to different conditions, such as straight bars, rings of metal, spheres which had been uniformly heated and were then left to cool, and various other particular cases which he examined theoretically. Some of these have been tested by experiment, and some of the most celebrated experiments of this kind are those of the French physicist, Despretz. Despretz employed the metal bars which you see here; there is one of iron and two of copper. Holes were bored in them to be filled with mercury, and thermometers were allowed to rest in each of these holes—a number of finely graduated and tested thermometers which are used for such experiments; then one end of the bar is heated in a crucible, and the other end is left to cool in the air. The temperature of the different parts of the bar is read off when it has arrived at the permanent condition of temperature. Applying these observations to Fourier's theory, Despretz arrived at certain values of this constant K, which was supposed to be the conductivity of the different metals he was observing. These were very carefully made experiments, and acquired a great reputation, and are still quoted as amongst the most important determinations that we have.

But in the first place, I must point out to you that Fourier made two assumptions in getting at his formula, which were, of course, also made by Despretz in his calculations. One of these was, that when a hot body is cooling in the air by radiation and convection the amount of heat which escapes in a unit of time is proportional to the difference in temperature between the cooling body and the atmosphere around it. This is what is called Newton's law of cooling, but from the experiments of Messrs. Dulong and Petit we know perfectly well that it is not quite correct; but they had not discovered their law before Fourier published his theory, and fortunately so perhaps, because otherwise it is doubtful whether Fourier would have ventured to have undertaken such a frightful calculation as the theory would have required, without his own book to start with, the book founded on this simple law. He also assumed that this law maintains in the interior of these bars, that the flux of heat is directly proportional to the difference in temperature at unit distances along the bar. This is what we assume in stating this formula of Fourier,

that the flux of heat is directly proportional to $\theta_1 - \theta_2$. This law is not exactly correct, but tolerably near so, and it will be sufficiently accurate if we still define conductivity in the way I have done, and for that the conductivity is not a constant quantity, but varies slightly with the temperature. Nevertheless, these assumptions were sufficiently near the truth to give very accurate results with proper apparatus. Some experiments of the same kind had been made previously by M. Biot. He had employed bars of different metals about eight feet long, and the results which he got were perfectly different from the results which Despretz got, and in a most valuable examination of our knowledge about conductivity, which was made by Professor Kelland in 1840 in a report to the British Association, he was led to investigate the different causes which might contribute to this difference between Despretz and Biot, and he arrived at the conclusion that the only cause could be that M. Despretz's bars were too short, and that their extremities were not at the temperature of the air, that is, that the bars were not sufficiently long to answer the conditions of Fourier's problem. He said Despretz does not mention what the length of these bars is that he used, and I myself have not the means of examining them. This has been the condition in which Englishmen, remaining at home, have been ever since this report of Professor Kelland in 1840 until the present summer, and this is the first opportunity we have had here of determining whether Kelland's suggestion was true; but I have not the slightest hesitation in saying on seeing these bars of M. Despretz, that they were very much too short to have given that permanent state of temperature such as was required by Fourier's theory. Consequently, this extremely acute suggestion as to the difference in the results of M. Biot and Despretz is, I think, wholly confirmed by the exhibition of this apparatus which we have here.

Fourier applied his theory to a variety of special cases. He pointed out also that since the temperature of the earth increases as we descend to greater depths under the surface, it follows that heat must be continually being lost from the body of the earth, because this difference of temperature could not be maintained unless there was a flux of

heat from the hotter to the colder part, and consequently, we may rest assured that there is continual flux of heat from the interior to the exterior of the earth itself by radiation into space—that the earth in fact is getting gradually cold. Geologists and natural philosophers have at times come into collision on this matter, and various opinions have been held about it. Sir Charles Lyell supposed that by the aid of chemical and electrical action in the interior of the earth a continual supply of heat could always be maintained, and that this could go on for ever, so that there would always be a gradual flux of heat from the interior to the exterior, which was always to be compensated by this chemical and electrical action going on in the interior. This, I need hardly say, is completely opposed to all our knowledge of the theory of heat and to Sir William Thomson's grand theory of the dissipation of energy. It might be possible to account for the difference of temperature as we descend on a chemical hypothesis, provided it was only a local fact, but it is a fact which is, to the best of my belief, perfectly universal all over the world, that the deeper you descend the greater is the increase in temperature, and consequently we must assume that there is this regular flux of heat from the earth, and that the earth is gradually cooling. Following out the analysis of Fourier, Sir William Thomson was led, about twelve years ago, to calculate the condition of the earth, as to temperature, backward, for a vast number of years. If we know what is the conductivity of the rocks, that is, K, and if we know the difference in temperature at an interval, say D, of 100 feet, then we can find out the flux of heat through a given area of the earth's surface in a given time, simply by this formula, and consequently we can measure the quantity of heat which is being lost by the earth every year, and can sum this up by a mathematical process and find the quantity of heat lost in past ages, and so we can find the temperature which the earth has had at different epochs in its history. Following out this idea, Sir William Thomson was led to conclude that, if we assume 7,000° F. to be the temperature of molten rocks, then 98 million years ago the earth was in the condition of a molten mass of rock with a crust just beginning to form upon the outside. This is a very important conclusion, geologically as well as

physically, because geologists, perhaps accepting too completely the theory of uniform action throughout the world's history, were led to conclude that the earth must have existed millions of millions of years in order to produce the effects that we find on it. But Sir William Thomson has shown that really this is not the right way of proceeding, but that we must conclude that in past ages there were much more violent differences in temperature, more violent storms, that the actions of nature upon the earth's surface must have been more violent owing to a much greater temperature; and this explains how geologists have assumed too long a period for the existence of the earth. There is very little reason to doubt that the statement of Sir William Thomson is perfectly correct, that the earth cannot be older than 100 million years, if we assume its birth to be at the time when the crust was beginning to form on its surface.

There was another very important problem which was attacked by Fourier in his splendid work, which is the case when heat is applied to a body in varying quantities at different times. For example, the earth is receiving heat constantly from the sun, but the quantity of heat which it receives varies in different parts of the surface at different times of the year and different times of the day. Consequently, at any part of the earth's surface we find heat being propagated downwards by the sun's action, the surface is heated, and this constant high temperature is gradually propagated downwards. About noon is the daily maximum heat that it sends down into the earth, but owing to the bad conductivity of the earth's substance, only for a very short time is it perceptible. But the difference between summer and winter is very perceptible to a very great depth, and we find as we descend lower and lower that the maximum of heat in the earth's surface becomes retarded. Thus it takes about one year for the maximum of temperature, that is, the summer temperature, to descend to a depth of 50 feet. At present, at the depth of 25 feet below the surface, it is winter, and at a depth of 50 feet it is only last summer. The retardation of the maximum temperature at different depths is directly proportional to the time, but it also depends on the conductivity of the rocks through which the heat is passing;

consequently, if we measure the time which the maximum takes to travel down to a given depth in the earth's surface, we shall be able to measure the conductivity of the rocks.

Again, as we descend to greater depths the variation in temperature at any particular depth becomes less and less. Thus, at a depth of two yards, the daily variations of temperature are quite imperceptible to any instrument that we may employ. But the annual variations in temperature can be recorded to very much greater depths, in fact to about nineteen times that amount, or to about 38 yards. Now, this is a very important point to observe, that the depth to which the annual variation of temperature goes is nineteen times as great as the depth to which the daily variation goes. This is completely in accordance with the theory of Fourier. He shows that the depth to which a periodical fluctuation of temperature will descend is proportional to the square root of the period of that variation. Now, the periods of variation in this case are one day and one year, and, therefore, the square roots of the period are in the proportion of the square root of 1 to the square root of 365, therefore, we ought to find that $1 : 19 :: \sqrt{1} : \sqrt{365}$. If you multiply 19 by itself you get 361, which is almost exactly the ratio of a year to a day. Some experiments were instituted at Edinburgh, by the late Principal Forbes, with different thermometers, which were sunk to various depths in the soil in different districts about Edinburgh. One series were sunk in the rocks forming the Calton Hill, on which the Observatory stands. Another set were sunk in the Experimental Gardens to conceal the sand, and a third set were established in a sandstone bed. These thermometers were placed at different depths, going down to 24 feet, and it was found that the retardation of the maximum and minimum followed exactly the laws of Fourier, and also that the variations in amplitudes of difference in temperature followed Fourier's law as we descended to greater depths. Other observations of the same kind have since been taken up in different regions, and a very valuable set has been put at Greenwich with exactly analogous instruments. These results were reduced by Principal Forbes, and afterwards a much larger series of them were reduced with every degree of accuracy possible by Sir

William Thomson, with the object of arriving at a true value of the conductivity of these different substances. The result was that the conductivity is obtained for these different kinds of rock with, perhaps, greater accuracy than the conductivity of any other substance which is here, and the results are as follows:—Of trap rock the conductivity is ·00415, that is to say, if we had a depth of trap rock one centimeter thick and kept the opposite faces of it at a difference of temperature of 1°, then the flux of heat through every square centimeter in that block of rock would be in a second of time sufficient to raise the temperature of a gramme of water through ·00415° C. For sand, the number is ·00262; and for sandstone, ·01068.

In the matter of metals the most important observations which have been made are those made on this bar, which were made at Edinburgh by the late Principal Forbes. This bar, which is made of as pure iron as it is possible to get, was mounted on two pivots so as to be free to radiate in all directions. A crucible is attached to one end, into which solder was put and kept continually melted, and it was found by continually adding some of the solid metal, keeping some unmelted always in the crucible, the temperature could be maintained with great accuracy for a good many hours. Then thermometers were placed in holes in the upper surface, and the temperature recorded. The method of reduction was one completely unfailing, and did not involve any theory whatever, and thus these results became a test of the accuracy of the assumptions that Fourier made in his theory. I will try and explain briefly the general method employed in reducing the observations. There was another bar of exactly the same material as this, a short bar, which was heated up to an intensely high temperature and then allowed gradually to cool. A thermometer was placed in this bar exactly as in the long bar, and the temperature as it cooled was continually observed; and then if results were jotted down upon a curve or in a table we should be able to tell what was the loss of heat by radiation and convection in a minute of time at any given temperature from direct measurement. Then suppose we take a base line and mark it in divisions to represent differences of temperature as we pass along, say, 10, 20, and 30 degrees above the temperature of the air;

at these points let us raise lines indicating the amount of heat which escaped in a minute. Suppose it started at 50° above the temperature of the air, it would be observed that there was a loss in one minute of a certain amount of heat represented by a line of a certain height, but when it got to 40°, the loss of heat in one minute was less, and only extended up to the height of the next line, and so we go on until the loss of heat is nothing. Then, connecting these points we get a curve representing the loss of heat at every temperature of the bar, and that is what we required to reduce the observations. You will notice at every point of this bar a certain amount of heat is being lost and passing through by conduction to the cooler parts. What we want to measure is the quantity of heat which is passing through the bar at any point. Now you will notice that by the time the heat has passed through the bar it is all dissipated before it can reach the end, because the temperature is the same as that of the air, and consequently all the heat which passes through any section is dissipated before it reaches the end. Consequently, if we wish to know the quantity of heat which is passing through the bar at this temperature all we have to do is to measure the quantity of heat which is lost with these different temperatures in the course of a minute. Suppose at one point the difference of temperature between the bar and the air is 50°, we know what heat is lost from there, and by adding the quantity of heat lost at every point we know that is the quantity passing through the section from which we started. This method, which was employed, involved most laborious calculations, and it was only after many years that these were completed and led to extremely satisfactory results, showing that the conductivity of iron is not constant, but that it diminishes with the temperature, as the temperature rises the conductivity diminishes. This was a very remarkable result, not only because it was different from what had hitherto been believed, and from what Fourier had assumed, but also because of its remarkable analogy with electricity. The same experimenter, a great many years before, had pointed out the most remarkable fact that the different metals lie in the same order according to their conductivity for heat as they do with respect to their conductivity for electricity. Those

that are the best conductors of heat are the best conductors of electricity. This, so far as we can be certain, is true without exception. He was now able to show this still more remarkable fact, that the analogy between heat and electricity holds good in this matter also, the greater the temperature the less is the conductivity of metals for heat, and also, as Mr. Mathieson has shown, the less is the conductivity of these metals for electricity, and the variation is very much in the same proportion. This simply stands as a remarkable fact which has not been explained. Professor Tait has been led from theoretical considerations to conclude that the resistance which is opposed to the passage of heat or electricity is directly proportional to the absolute temperature of the substance.

Here I will draw your attention to a similar bar of German silver which has since been employed by Professor Tait, also at Edinburgh, and he has made a vast series of observations of the same kind as those made on iron, and it is only the difficulty and labour of reduction that has prevented us, as yet, from having results. These results are looked forward to with great interest, because German silver is a substance whose conductivity for electricity varies very little, and if it were shown to vary very little for heat, that would be another very great step in our knowledge.

I should have liked to have mentioned some of the methods which have been employed for determining the conductivity of bad conductors besides those made on the conductivity of the soil; but I shall have very little time for that, and will only devote a short time to explaining the method that I employed some years ago for determining, as a very important quantity, the thermo-conductivity of ice. The idea of the method was originally suggested by Sir William Thomson, namely, to artificially freeze water and see the rate at which the ice is formed. That will give you a means of measuring the quantity of heat which is passed through from the water to the freezing mixture, and you will have, therefore, a measure of it in the quantity of ice which is formed. There was a large tin vessel made to contain a quantity of snow in order to keep the instrument at an uniform temperature. In the interior on a tripod stand is a cylinder, also tin, about halfway up. It

was filled to a certain line with water, and above that was placed another cylinder containing a freezing mixture. Through the centre of this a hole was pierced which allowed a rod to descend with a cylinder on its base, and by means of raising the rod we could bring the cylinder in contact with the ice which was formed below, and so by reading the position of the rod on the scale could actually measure at different intervals the amount of ice which had been formed. The experiment proved rather laborious, and sometimes extended over a period of twenty-four hours, almost unceasing attention being required the whole time; but the results were extremely accordant, and it was found that the formation of the ice was exactly in accordance with theory, and, what was very fortunate, by employing fresh snow it was found possible to keep the temperature perfectly constant; simply changing the snow every hour or two, the temperature was found not to alter at all. You may notice the material point of this is the way in which the temperature is kept always constant at the lower surface. There it is always 0° C., because it is always at the boundary between the ice and the water. The result of this experiment was to show that the conductivity of ice in the same units which we have employed before, is ·00223. M. Lucien de la Rive, by an extremely ingenious method, but which was not quite so correct, arrived at the result of the conductivity of ice as ·00230, and I think the remarkably close accord between these two results and the complete accordance between a large number of experiments made with this apparatus may make us feel certain that the conductivity of ice is one of the best determined quantities we know. Consequently, we have as standards of the conductivity of heat the kinds of soil I have spoken of, also the conductivity of iron bars and the conductivity of ice, and these are the substances about which we know most.

A large number of experiments have been made by different philosophers to determine the conductivity of a number of bad conductors, but it must be confessed that none of their methods have been entirely unobjectionable, and in few can very great confidence be placed. Experiments have been tried following out this idea of freezing water through some bad conducting substance, and measur-

ing the quantity of ice which is frozen in a given time, say an hour, and then the quantity frozen is a measure of the quantity of heat passed through. But these experiments can hardly be considered as extremely accurate any more than can those made by several other philosophers. There is a great difficulty in keeping the temperature of the two surfaces of the substance absolutely constant and exactly the same as what you are measuring. For example, if I separate two vessels of water by a wall of metal and let one vessel be at a temperature of, say 80° C. and the other at a temperature of 10° C., it would be impossible to maintain that relation between the water close in contact with the sides of the tin vessels. The metal is such a good conductor that it would be an uniform temperature throughout, and there would be a thin layer of water on each side of it, and these layers would be of exactly the same temperature. What we require is to have the temperature of the water in contact with the substance differing by measured quantity from the water in contact with the other surface of the substance. This has been the great difficulty, and consequently our knowledge of the conductivity of bad conductors has not been very certain hitherto. A large number of results have been collected, and they have considerable value, as you can judge for yourselves. The agreement between different experimenters, if you consult this excellent book of Professor Everett's—an illustration of the centimeter, gramme, second system of unit—you will find a large number of results on conductivity classed together, and if you care to examine it you will see some results I have mentioned to-day spoken of.

Some experiments made quite lately under the auspices of the British Association by Professor Herschel, not only on heat, but also on measuring the conductivity in the laboratory, it is expected will give better results than we have had hitherto. You see, then, that a great deal of labour has been spent both on theory and in the experimental determination of conductivity, but a great deal has still to be done, and it is very desirable that some simple and easy method should be found for measuring the conductivity of different substances. It is of immense practical importance, now-a-days, when non-conducting cements for roofing materials are being constructed, and it

is indeed necessary to have some means of measuring the conductivity of such substances.

I will conclude by alluding to two extremely ingenious pieces of apparatus which are exhibited here, which illustrate a very important point. This is one founded on a principle originally employed by M. Senarmont for measuring the conductivity in crystals. He found that the conductivity of crystals varied in different directions. I will explain the application of this instrument, which is on the same principle. There is a disc of glass upon which the substance to be examined is laid, above it there is a flat vessel through which water can be made to flow in order to keep it at a constant temperature. There is a platinum wire connected with the terminals of a battery passing down and ending in a small bead of platinum. The bead of platinum at the centre can be lowered down on to the surface of the piece of rock. The rock is covered with wax, and when the platinum is in contact with the rock, and the wires are connected with the electric battery, this has the effect of heating the platinum to an intense heat, and consequently melting the wax on the surface of the rock. Now the distance to which it will melt this wax depends solely on the conductivity of the rock—the greater the conductivity the greater the distance to which the wax will be melted. But you will notice in all these specimens that instead of having a circle of melted wax round the plate which had been heated there is an ellipse —an oval curve—showing distinctly that the conductivity is greater in one direction than another. This instrument is exactly on the same principle as Senarmont's, and the results are exactly of the same nature as those arrived at by him in the case of crystals.

The conductivity of liquids has been very little investigated. There is great difficulty in it, owing to the convection currents, and also owing to the amount of radiation which passes through transparent substances. Here is a perfectly new instrument invented by Dr. Guthrie, which promises to give a very good idea of the relative conductivity of different liquids. The principle of it is extremely ingenious. There are two cones which are separated by a very small distance. The upper one is connected with these two tubes, through which steam can be

made to pass to keep it at an uniform temperature. The lower one is allowed to radiate off heat to the air. It gets heat by its proximity to the cone above. A certain amount of liquid is sucked up from a vessel below into a tube which is in connection with the lower cone, and this lower cone thus acts the part of the bulb of an air thermometer, and if it be heated very much the liquid in the tube will be forced down. If it be heated only a little it will be forced down very slightly, so that you have a measure of the temperature of this lower cone. The interval between these two cones is filled with a liquid to be examined; then the temperature of the upper cone being always 100° C. the conductivity of the different liquids is measured by the depth to which the liquid is driven down, that is to say, by the temperature acquired by the lower cone. The greater the flux of heat through this liquid the greater will be the temperature of the lower cone, and the temperature is measured by the height of the liquid in the tube. There is a scale at the top for measuring the distance between the two cones. If that distance is kept constant as we employ different liquids we shall be able to arrange the liquids which we examine in the order of their conductivity, those which are the best conductors being placed above, and those which are the worst being placed below. From these experiments it appears that water is one of the best conductors among liquids. Convection plays a very slight part there, or none at all, because the upper surface is the one which is heated. Despretz made some experiments with a large cylinder of water which he heated from the surface, and arrived at a rough approximation to the conductivity of water, but one which it is very desirable should be repeated. In fact, experiments have been made since then, but they have not been published, though it is very desirable that we should have a knowledge of the absolute conductivity of water.

THERMO-DYNAMICS.

BY PROFESSOR FORBES.

It is a remarkable fact that many truths in physics after they are well known appear to us to be almost axiomatic, although it required ages before they were thoroughly appreciated as scientific facts. For example, nothing would seem simpler and more evidently true to those who have acquired a fundamental knowledge of science than the principle that the planets in revolving round the sun require some force to attract them towards the sun in order to make them complete their path, whereas for ages people could not get it out of their minds that it was necessary to have a force behind the planet driving it round with a sort of vortical movement. Take again, the celebrated facts enunciated by Galileo, such as this, that a heavy body takes as long as a light body to fall to the ground from a given height. This also seems to us with a slight knowledge of physics to be perfectly certain and to require no proof.

So it is with the dynamical theory of heat. For many centuries past there have been men who looked upon heat as necessarily caused by the motions of molecules or particles of the body which is hot. But men's minds being firmly occupied with the theory that heat was a substance, it became a matter of difficulty to eradicate this notion, although now it appears to us to be perfectly axiomatic that no effects such as those produced by heat could be produced simply by matter which is not in motion.

The proofs on which the dynamic theory of heat rests are of various kinds, and I shall commence by simply pointing out some of those which are less directly included

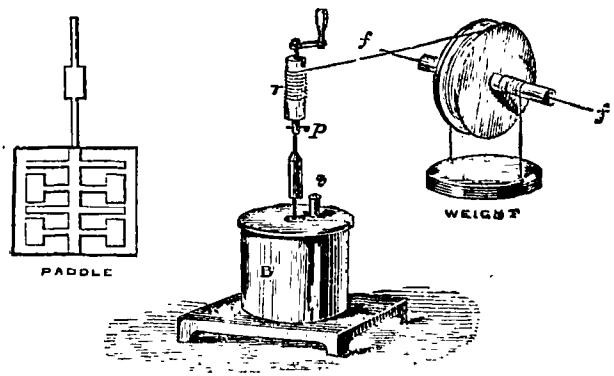
in the reasoning which has led finally to the adoption of this theory. I mentioned some few weeks ago, in a Lecture on the Radiation of Heat, what a striking proof the theory of radiation and of the transmission of heat by undulations through ether gives of the idea that heat in a body consists in the motion of its molecules; because in order that these vibrations which are transmitted through the ether should be originated, it is necessary that something, such as the molecules of the body, should be in agitation to communicate these vibrations. The most striking evidence of the truth of this theory is that in connection with gases which was first disclosed by the discoveries of Dr. Graham when he enunciated the laws of the diffusion of gases. Graham found that all gases have a tendency to permeate each other, that is that one gas tries to pass through and permeate the substance of another, and even where the two gases are separated by some partition, say of plaster of Paris, the gases on the two sides of the plaster of Paris have a tendency to pass through. Moreover, Graham found that the velocity with which these gases pass through the plaster of Paris is different, and depends on their density; the less dense gas passing through the most quickly.

Suppose I take a jar and fill it with a light gas such as coal-gas; then if I place the jar full of gas over the plaster of Paris partition we shall have common air below the partition and coal-gas above. The consequence of this will be that the coal-gas will tend to pass most quickly through and the air will pass slowly out through the plaster of Paris, and so you will notice a depression on the level of the liquid in the tube. To perform this experiment I will simply fill this jar with coal-gas and then place it over the plaster of Paris partition, and you will see by the descent of the liquid the rapidity with which the coal-gas penetrates into the cell. The tube with its funnel-shaped end is now nearly full of gas, and on removing the jar the air will penetrate into the tube and the gas will pass out again. The gas will pass out quicker than the air passes in, and consequently the pressure will be diminished, and you will see the liquid in the tube rising. The first great steps towards the proof of the theory of heat lay in the experimental determination of the fact, that wherever

mechanical energy or any other physical force is transformed into heat, there is a definite proportion between the mechanical force and the heat which is produced. These experiments were conducted in the first place to a great extent by Dr. Joule of Manchester. There are a vast number of ways in which we can transform mechanical energy, or chemical affinity, or electrical action into heat, and Dr. Joule attempted to prove this in a variety of different ways. The first apparatus which he employed in the year 1840 is on the table. There is a powerful electro-magnet which can be rendered highly magnetic by a current of electricity. Between its two poles he caused this coil of wire to rotate with great rapidity, and when the coil of wire rotates between the poles of a magnet a current of electricity is generated through the wire, and this generation of the current of electricity causes heat in the wire, and the heat produced in the wire is the exact result mechanically of the work in turning it. But the presence of the magnetic poles causes a resistance to the motion of this coil, and if we can measure the amount of heat developed in this wire we shall know the amount of resistance which has been opposed to the motion of the wire. Joule filled this tube with water, and after it had been rotated for some time, and the resistance it overcame had been measured, he measured the increase of temperature, and so arrived at a rough approximation of the mechanical equivalent of heat.

He afterwards extended the experiment in a variety of ways, and perfected the methods more completely until he was able to get exactly the same result every time, when he varied the conditions of the experiment. The most perfect apparatus he was able to construct is exhibited here. This is the celebrated water-agitator which was employed by Joule, the first results of which were published in 1843. There are a certain number of fans which are capable of rotation. These fans or paddles rotate in such a way that they cause the water to be extremely agitated, owing to the fans having to pass through these openings in the fixed radial planes. This apparatus was placed in a copper vessel which was filled with water. Then on the spindle which you see here two threads were twisted in the same direction a great many times. These threads were drawn away and passed over two pulleys in opposite

directions and heavy weights weighing from 10 to 30 lbs. were attached to the extremities of the strings. When the apparatus was in perfect order there was very little friction, because the strain of the string is equal in the two directions. The quantity of friction there was he was enabled to measure by an extremely ingenious process with considerable accuracy. He let his weights descend

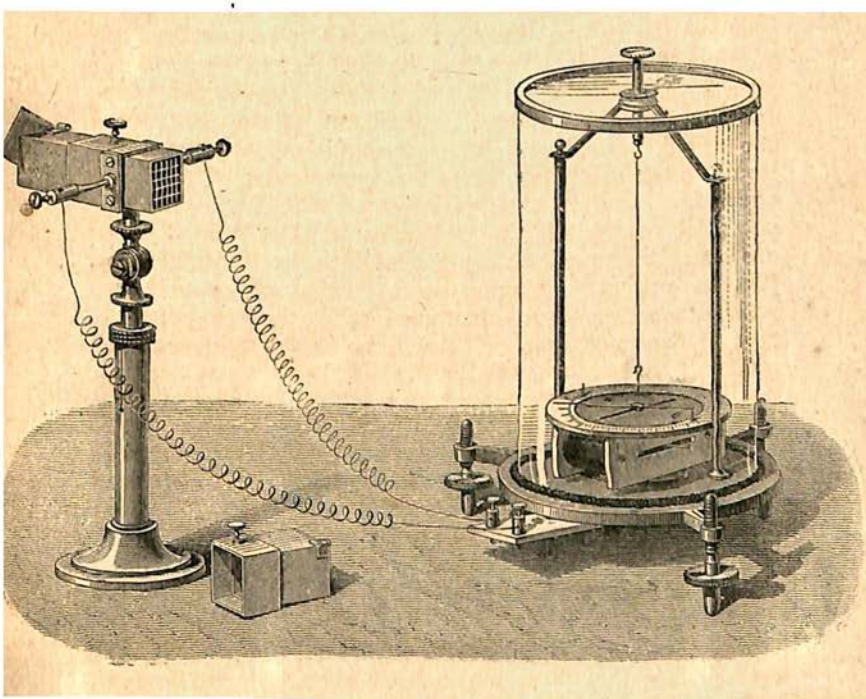


to the floor from a distance of five feet, then he wound the string up again without turning the paddles at the same time and let the weights descend again, and repeated this operation twenty times for each experiment. After that he measured the temperature of the water, and knowing the weight of water there was there, he knew the amount of heat that had been generated. He also knew the amount of mechanical force which had been employed because these leaden weights had descended 20 feet five times. He also calculated the amount of friction due to the resistance pivots. Great precautions were taken to prevent any error arising from the heat of the body reaching the apparatus or from the heat escaping from the vessel containing the water; and measurements were made to find out how much heat did thus escape, and allowance was made for it. Every possible correction having been thus applied, Joule eventually arrived at the conclusion that the mechanical equivalent of heat is 772 feet lbs.,

that is to say, if a weight of 1 lb. be allowed to fall from a height of 772 feet, it would generate heat when it stopped sufficient to raise 1 lb. of water 1° Fahr. These experiments were very important because they were entirely of a new order. Rumford and Davy in previous times had almost as clearly as Joule pointed out that there must be a mechanical equivalent of heat although they gave no exact experiments proving that this was the case; but philosophers up to the time of Joule had never admitted that the friction of liquids or the friction of gases could produce these differences of temperature, as was known to be the case with the friction of solids. Rumford's celebrated experiment consisted in measuring the increase of temperature in the boring of a cannon, but people who supported the theory that heat is matter said that the material of which the cannon was composed changed its state when it was bored, and consequently that there might be some evolution of latent heat. But in the case of the water which was employed by Joule there is not the slightest doubt that the water at the end of the experiment is in precisely the same condition as at the beginning, with the exception that it is so many degrees hotter.

Now in order to show you that this is really true with respect to gases, which is perhaps less easy to grasp, I will show you experiments which will illustrate the action of gases in producing heat by friction. Here I have a delicate galvanometer which is in connection by these wires with a thermopile, the most delicate means of measuring temperature which we possess. As soon as I heat one side of this thermopile the needle is driven away in one direction, and if I were to cool it by putting a block of ice near it, the needle would move in the opposite direction. Now if I take a pair of bellows and blow gently on the face of the thermopile there will be sufficient friction of the air to produce a sensible motion of that needle. You see as I blow the face of the pile is gradually warmed and the needle moves to one side, showing that there is a certain amount of heat generated by the friction of the air against the pile. In this case the work is done at the point of contact between the air and the pile; but if I employ air which is intensely compressed, such as that which is inclosed in this vessel, so soon as I let the air

escape, the air does a vast amount of work in creating vortical whirls; it causes the air that issues to circulate in vortical movements with great velocity, and in order to do this it expends a vast amount of work, and the consequence is that the air in doing this working cools to a very great extent, because instead of having work done upon it, it is doing work itself, and in order to do work it must absorb heat, and the heat absorbed is the exact



equivalent of the work which the gas does. You see the needle move sensibly away to the other side, showing that cold was produced by the work which was being done by the air in issuing from that orifice. There are a very large number of other ways in which heat can be generated without the performance of mechanical work. There is, for example, when chemical combinations take place, a development of heat, although the operator is exerting no

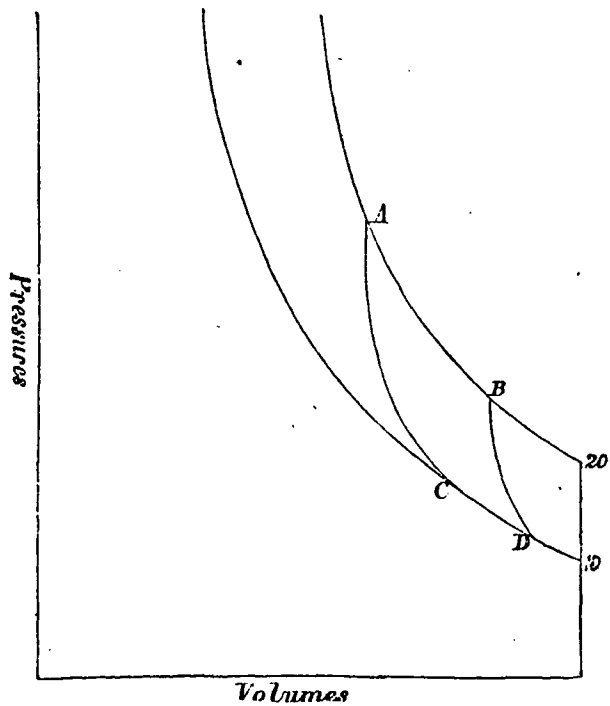
great mechanical force. Or, again, when a body changes its condition from the liquid to the solid form, a considerable amount of work is done, and that is shown by a change in the temperature. For example, we may employ such a solution as this of glauber salts which is supersaturated, that is to say, it contains more salt than is natural to it in its present condition, and the reason why it does not crystallise is that the solution is in a sort of passive state; it is ready to crystallise, but it has received no inducement to crystallise in one direction more than in another. If, however, I introduce into the liquid a small piece of the crystal itself, then crystals will be formed about this piece, and there will be a sudden solidification of the contents of the vessel. Now when this substance changes from the liquid to the solid state there is a contraction of the mass which is equivalent to a variation of what I may call the potential energy of the molecules of this mass. They are brought closer into contact with an evolution of heat, just as you may imagine an evolution of heat if two planets meeting each other were suddenly brought to a stop by their collision. This will be shown by the movements of the needle as the liquid solidifies by crystallisation. So it is always; whenever there is that change of state there is a waste or gain of heat from the substance.

Now I must pass from this portion of the lecture as shortly as possible, which is an enunciation of the first law of thermo-dynamics—which is illustrated by these experiments we have seen, and which was the great labour effected by Joule; but I recommend you all to read the first account of his work which he published in the *Philosophical Transactions* for 1850.

But now I will pass on and try to explain some of the very remarkable facts which have been brought to light by the study of what is called the second law of thermo-dynamics.

I suppose that we have a piston working in a cylinder and gas, then if we exert a pressure upon the piston we reduce the volume of gas, and the greater the pressure the less is the volume occupied by the gas. If we increase the pressure very slowly so that the temperature remains uniform we shall find that the substance will have its condition in every state represented by such curves as

these two lines which I have marked at 10° C. and 20° C.; that is to say, suppose a substance has a pressure of six atmospheres, or any other unit of pressure, and then that this volume is $2\frac{3}{4}$ cubit feet, then the condition of the substance at that time will be represented by the point A. Then let the pressure gradually be diminished until it reaches $4\frac{1}{2}$ cubic feet, then it will come to this point B, and

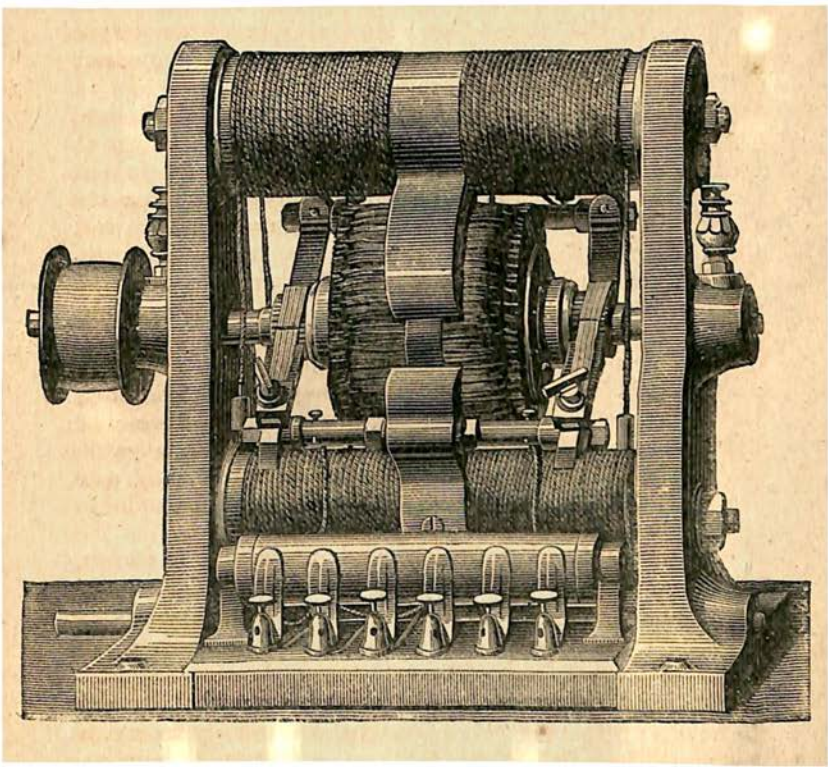


the line of that curve will represent the condition of the substance there, and its volume will be represented by the perpendicular distance of the point from the line of pressure; that is to say, about $3\frac{3}{4}$ cubic feet. If, on the other hand, we employ a temperature of 10° the state of the same substance will be indicated by this curve in exactly the

same way. But I want to point out to you that we can alter the volume of a gas in different ways. In the case I am speaking of we may increase the pressure so gradually that it has not time to get warm, but if I were to compress the gas very suddenly it would generate a certain amount of heat by the mechanical work I was doing, and consequently this heat would tend to expand the gas and prevent a given pressure from reducing the volume so much as in the previous case. Thus if I compress the gas without letting any heat escape, the increase in pressure corresponding to a given diminution in volume will be less. Such a state is represented by two curves which I have called *adiabatic* curves. If I increase the pressure from C, keeping the temperature constant, the curve goes up, but if I do it so suddenly that no heat can escape, then the pressure is greater than would be indicated by that line. In order that you may see that this is really the case, that we can generate a considerable amount of heat by the application of pressure to a gas, I will take a little cotton-wool moistened in bi-sulphide of carbon, and by introducing it into this glass syringe and taking it out again that introduces a certain amount of vapour of bi-sulphide of carbon. Now if you watch carefully as I compress it, you will see a flash indicating the amount of heat generated by my compression. If I had done this more gently, and had compressed it so slowly that the heat was able to escape, and it was kept at a uniform temperature, then the pressure when I had reduced this volume to a certain amount would not be so great as in the case I have just shown you. In other words, if we do it slow enough we are following one of these isothermal curves, but if we do it very rapidly we follow an adiabatic curve.

Now in the consideration of the second law of thermo-dynamics there is one thing we have continually to speak of, and that is the employment of a reversible engine. The idea of a reversible engine was introduced by Carnot in 1824, and has proved of enormous service in the principles of thermo-dynamics. I will show you an example of a certain engine whose action can be reversed, but nevertheless it is not what Carnot calls a reversible engine. Here I have a Gramme machine, for generating electricity. By rotation by a bell over the left-hand axle I cause coils

of copper wire to rotate between the poles of a large magnet, and the consequence is that electricity is generated in these coils, and the electricity can be conducted along these wires, and can be exhibited by heat in a piece of platinum wire. You see that there is a considerable quantity of electricity generated by the performance of mechanical work. But if I increase the rapidity of rotation so as



to melt the platinum wire, as soon as the wire is melted the machine almost runs away, because then I have taken away the resistance which was producing this heat; I am no longer producing an electrical current, and consequently I have not to spend so much force in turning the machine. That difference in the mechanical exertion which I was using was indicated by the amount of heat which was

generated by the electric current. Now I will produce the reverse effect, and show you that if I drive a current of electricity through these wires I shall be able to produce a mechanical effect simply by the reverse action. In this case a current of electricity will be passed through these coils which lie near the poles of the magnet. The consequence of this is that the coils of wire with the electricity moving through them will have a tendency to rotate past the poles of the magnet, and thus magnetic energy will be exerted. As soon as contact is made you see the wheel will rotate in the reverse direction.

Now this is truly what we might call a reversible engine, because in the first place I exerted mechanical effort to produce an electric current, and in the second place I cause an electric current to produce the reverse mechanical effect. But in thermo-dynamics this is not what we call a reversible engine, because there are certain effects produced by the mechanical action which are not reversed when we connect it with an electric battery. For example, when I turn the machine, the two wires being in contact, there is a heating the whole length of this wire, and consequently when reversing this effect, when I connect these wires with the battery, the wires ought to be cooled. But this is not the case; they are heated as much by the battery as they were by the mechanical action, and consequently the machine as exhibited here is not a completely reversible engine.

Another case will occur to you when you take the action of a steam-engine. Suppose, in place of giving heat to the steam and making it do mechanical work, that you were working an engine in the reverse direction, you would exert mechanical energy in compressing your steam and in converting it into water, and in this way you would reverse the action; instead of gaining mechanical energy from heat you would be gaining a certain amount of heat from the expenditure of mechanical energy. But in this case also there is waste of heat from conduction and other means through the cylinder, and consequently the steam-engine also is not a reversible engine in the thermo-dynamical sense of the word.

Now let me try and give you an idea of a truly reversible engine. Supposing I had two vessels at different

temperatures; suppose this vessel filled with ice, and here I have the room at a certain different temperature. Let me start with this cylinder, and suppose that the temperature was 10°, and I increase the heat to the condition indicated by the point C on the diagram. Let me now compress this suddenly so that no heat escapes. I bring it to the point A; then while it is still compressed very suddenly I have it at a high temperature. I ought to have pointed out that the temperature in the vessel was 10°, and that by suddenly compressing it I raised it up to a temperature of 20° at a great pressure; then you may diminish the pressure gradually, keeping it at a constant temperature. Then it will expand until it occupies some position on that line indicated by B. Then let me suddenly expand it a good bit more without allowing any heat to escape; it will grow cool because it has exerted a mechanical effect in driving out the piston, and since no heat escapes its condition will pass along this line and it will come to the point D. Let this go on until it reaches the temperature of this vessel, 10°; then finally let me put it in this vessel, and keeping it at a constant temperature gradually increase the pressure until I reach the original volume which it occupied, which was indicated by the point C. Thus I have completed a cycle of operations which you will very easily see is perfectly reversible, because I have only taken in or given out heat during two of the operations, when passing from A to B, and from D to C. I took in heat when passing from A to B, and gave it out when passing from D to C. Along the other parts of the curve there was no transference of heat, because I performed the operation so suddenly. Now since the transference of heat always took place at a definite temperature, without the temperature changing, it is perfectly easy to reverse that operation and to pass from B to A by compressing the substance gradually without any heat being allowed to escape. This is exactly the reverse operation which was performed before. You will easily see therefore that all these four operations are perfectly reversible, and therefore such an engine, if it could be made, would be a perfectly reversible engine.

Now the remarkable point connected with this reversible engine is, that a reversible engine is the most perfect kind of engine that can be imagined. If it were possible to

have an engine more perfect than this reversible engine, let me employ that engine to drive this reversible one, of which I have been speaking, in the reverse direction. Then, since it is more powerful than this reversible engine, it will transfer more heat from this cool part—from every part of it beyond C D to the body, which is at a temperature of 20°; and therefore the most perfect engine will cause this reversible engine to transfer more heat from C D to A B than that more perfect engine is utilizing; in other words, we should have perpetual motion by the employment of such an engine. The more perfect engine would be using this reversible engine in order to store up a large supply of heat,—to carry heat from a body at a low temperature to a body at a high temperature.

Now we may state the second law of thermo-dynamics, either in the form that it is impossible by the aid of any dead matter to transfer heat from a source at a low temperature to a body at a high temperature; or we may say that it is impossible to produce perpetual motion. In either of these cases the reversible engine must be the most perfect one that we can produce. I am afraid that I shall not have time to go as fully into the diagram as I intended, but I will indicate some points about it which lead to most important conclusions. In the reversible engine the path A B C D is traced out, and we can also prove very simply that the area A B C D is proportional to the amount of work which is done by the engine. It is impossible for me to explain to you the reasoning by which Sir William Thomson arrived at this conclusion, but I will simply allude to the fact that Sir William Thomson has found that if we measure the temperature of a body by the area included between these two adiabatic lines from the points where they meet one another up to the isothermal line for that particular temperature, then we have an absolute measure of temperature which is independent of the substance which is employed to make our thermometer of; and also that this measure of temperature agrees very closely with the measure derived from an air thermometer.

Consequently it follows that the heat utilized in such an engine as I have been describing is to the heat which is absorbed from the boiler in the ratio of the difference of temperature between the refrigerator and the boiler to the

absolute temperature of the boiler, that is to the temperature measured from absolute zero, where there is no heat whatever. Consequently if we indicate the absolute temperature by measuring from the absolute zero, that is to say, from about − 273 C., and if T be the temperature of the boiler and S the temperature of the condenser, and suppose we absorb from the boiler an amount of heat equal to H, then the heat which it has utilized is $H \times \frac{T - S}{T}$.

There are a great number of other deductions which can be made from a study of this diagram. Some of the most important of these depend on the evolution of heat when we compress or expand different substances; and one of the most general conclusions arrived at is this, that in the case of all substances which expand with an increase of temperature we shall have an increase of heat when we compress them. But there are one or two substances which contract when heat is added to them; and these it is found give off heat or grow cool when they are compressed. Water, for example, between the temperature 0° C. and 4° C., is such a body; and it was pointed out by Joule that India-rubber when it is suddenly stretched instead of growing cooler grows hotter, and in consequence of this it was predicted theoretically that if stretched India-rubber be heated gradually, instead of lengthening it will contract. This is one of the most striking applications of the theory of the second law of thermo-dynamics; but perhaps the most striking one is the experiment of Prof. James Thomson, which was afterwards followed by his brother Sir William Thomson, in which he showed that the melting point of ice is lowered when we exert great pressure upon the ice. He deduced this from the fact that the water expands at the moment when it solidifies. I have here an arrangement by means of which perhaps I shall be able to show this. But it is a troublesome experiment, and my galvanometer has rather more resistance in it than is desirable to show it well. I have here two wires, one of iron and another of copper. There are two junctions of the iron and copper wires, one of which I keep constantly in a vessel containing ice and water to keep it at a constant temperature, and the other junction is passed over a block

of ice. I shall apply a weight in order to press this junction against the ice, and when I give a certain amount of pressure to the wire the pressure lowers the melting point of the ice, and since this ice cannot get cooler it must melt. The consequence of this is that the wire is gradually eating into the ice, and the greater weight we apply the more it will eat into the ice, but as soon as the wire passes through the ice the pressure by the wire is relieved and consequently the water will solidify, and thus we shall have the phenomenon of the piece of wire cutting its way into the ice and the ice being solidified in the channel through which the wire has passed. But more than that, the temperature at the place where this eating is being accomplished is changed, the temperature is lowered by the application of this process, and I hope by the arrangement of wires with this thermo-electric junction that we may just be able to see the alteration of temperature when we exert a considerable pressure upon the wire.

Now I will show you the experiment of stretching India-rubber, which is another almost as striking an illustration of the second law of thermo-dynamics. This India-rubber is suspended from a point above in the interior of a tube. The weight which stretches it has its base in contact with the short arm of this lever which points to zero on the scale. I can heat the India-rubber by holding a flame at the bottom of the tube in which it is suspended, and we shall find that the India-rubber contracts, as indicated by the motion of the pointer on the scale; so that there is not the least doubt that the application of heat to stretched India-rubber causes it to contract. In connection with this there are several curious considerations, to which I would call your attention. Here is one point which has not yet been investigated. This stretched India-rubber contracts when you apply heat to it, but if it were not stretched we know it would expand by the application of heat. What then is the amount of stretching required in order that the India-rubber might neither expand nor contract? This would probably depend on the temperature and a variety of other causes; but it would be extremely interesting if some one would investigate the subject.

I regret that I have been able to-day to enter only so

very shortly into this subject, for I should have liked, if possible, to have gone further, not only into the purely thermo-dynamical work which has been done, chiefly by Clausius and Sir William Thomson and the late Prof. Rankine, but also into the Kinematic theory of gases in which so much has been done especially by Prof. Clerk-Maxwell; but those who wish to study this subject further will find the theory extremely clearly stated in Prof. Clerk-Maxwell's admirable book on Heat; and there are some beautiful propositions which I hoped to explain to you to be found in Rankine's book on Prime Movers, which were published in the *Philosophical Transactions of the Royal Society of Edinburgh.*

The first law of thermo-dynamics is perhaps more easily understood, but the applications of the second law are so important that I have tried to spend a little more time on them than on the first law. I am afraid I have gone too rapidly over the subject; but it is a very extensive one, and I can only hope you will read up this portion of it which I have spoken of.

ON BALANCES.

BY H. W. CHISHOLM.

1. BEFORE proceeding to describe the several kinds of balances, and the mode of using them for scientific purposes I propose to call your attention to the principle of the balance and object of its use.

The balance, whether in its simplest form or with its most elaborate additions, is the instrument universally employed for *weighing* bodies, or, in more scientific language, for measuring their mass. For this purpose the weight of the body is compared by means of the balance with another body of known weight, such as a standard weight or a duly authenticated copy.

2. All weights are referred to one standard or unit of weight, and are stated in numerical terms of this unit, or of one of its multiples or parts. In this country the unit of weight is the imperial standard pound avoirdupois. It is made of platinum, and is in my legal custody as Warden of the Standards. It is kept, together with the unit of length, the imperial standard yard, which is a bar of bronze, in a fireproof iron chest in the strong-room of the Standards Office, 7, Old Palace Yard, Westminster. The standard pound is not allowed to be taken from its place of deposit except on very special occasions. But I can here show you a facsimile of it, a copy in platinum-iridium, composed of ten parts of iridium to ninety parts of platinum. This alloy is a much better material for a primary standard weight than platinum, as platinum is a soft metal and liable to injury, whilst platinum-iridium is harder than steel. Platinum-iridium has been selected by the International Metric Commission at Paris as the material for the new international metric stan-

dards—the kilogram, the unit of metric weight, and the metre, the unit of metric length; the object of the appointment of this commission having been to construct new primary standard units of metric weight and length and identical copies for all the countries of the civilized world. You may see specimens of this platinum-iridium exhibited by Mr. Matthey (Catalogue Appendix, Nos. 259*b* and 259*c*), showing also the peculiar sectional form of the new standard metre.

3. But I must proceed with a definition of weight. The *weight* of a body is the measure of the force of gravitation which the mass of our globe exercises upon the mass of all smaller bodies upon its surface, and in a line perpendicular to its surface, or, more properly speaking, to the surface of a still liquid upon it. *Gravitation* is the effect of the force of attraction which is inherent in all physical bodies, by which they are drawn towards each other in proportion to their mass, or the quantity of matter that each body contains. As shown by Sir Isaac Newton, attraction acts universally with a force varying inversely as the square of the distance from the centre of the mass, and with a velocity varying in proportion as the medium through which the bodies drawn is more or less rare.

4. If our globe were a perfect sphere, and of uniform density (I shall have presently to speak to you of *density* as respects the density of bodies weighed), the force of gravitation would be the same on all parts of the earth's surface. It is known, however, that the figure of the earth is flattened at the poles, and the best computations have determined this flattening, or the difference between the length of its polar axis as compared with its mean equatorial diameter, to be about $\frac{1}{304}$th part of the earth's diameter. The effect of this difference of distance from the surface to the centre of the earth is to diminish the force of gravity in passing from the pole to the equator; and it has accordingly been computed that a weight of 100 lbs. at the equator weighs more $100\frac{1}{2}$ lbs. at the pole, and more than $100\frac{1}{3}$ lbs. in the latitude of London. Practically, however, one could only test this difference of the force of gravity with a spring balance, and not with a beam balance, where the weights in each pan must both be equally affected by the force of gravitation.

I shall presently show, when describing the mode of scien-

tific weighing, in which the greatest accuracy is required, that allowance must be made not only for the different force of gravitation at different latitudes, but also at places differing more or less in height from the sea level. That the force of gravitation at different points of the earth's surface is also affected by the density of the earth underneath such points is beautifully exemplified by Dr. Siemens' instrument, the bathometer, as will be explained to you by himself.

5. With these preliminary observations upon the subject of weight, I will now proceed to the subject of balances, the more immediate object of this lecture.

The balance in its simplest form is a beam made to vibrate upon a centre of motion, with pans, or other contrivances for supporting bodies weighed, hanging from the extremities of the two arms of the balance. From the depression of either pan the excess of weight of the body placed in it is determined. Balances may be classed under two heads: (1) Ordinary balances, or scale-beams with equal arms, having the beam suspended at its middle. If an equal-armed balance is properly adjusted so that the beam is exactly horizontal when the pans are empty, the balance will be in equilibrium; and the balance will also be in equilibrium, that is to say, the beam will rest in a horizontal position, after equal weights have been placed in the pans. (2) Balances with unequal arms, in which the beam vibrates upon the centre of motion placed more or less near one of the extremities. These two classes comprehend all balances of precision, or scientific balances. Spring balances are also used, where the body weighed pulls down a spring to which a pointer is attached, moving on a graduated scale, and thus indicating the weight. But these balances are not balances of precision.

6. You may here see a good specimen of an equal-armed balance, made by Mr. Oertling, who has constructed most of the best balances of the Standards Department. It is made to hold a kilogram, or a 2 lbs. weight, in each pan. You may see that when the balance is set in motion, the pans being empty, the beam is exactly horizontal. In order that it should be so, it is necessary that the frame of the balance should be accurately levelled; and you may see that is done by two spirit levels placed upon it. The horizontal position of the beam is shown by the pointer at one of its extremities being in the middle of the graduated index.

I should explain to you that in all scientific weighings, or weighings in which special accuracy is required for determining the exact amount of difference between two bodies weighed, the weights placed in each pan should be so adjusted that the beam only oscillates very slightly, and the pointer does not range beyond the limits of the index. The adjustment is effected by adding to the lighter pan very small balance weights, the value of which has been accurately determined, until the requisite approach to equilibrium is attained. The difference of the two bodies weighed can then be read off on the index in divisions, the value of which is known.

We now arrest the balance, and place one of two equal 1 lb. weights in each pan, and again set the balance in motion. These are gilt bronze standard weights very accurately verified. You will see that the beam of the balance continues horizontal, as shown by the position of the pointer.

7. I shall next show you two specimens of balances with unequal arms. The first of these is part of a model kit, containing all the instruments necessary for an inspector of weights and measures, these instruments being made to pack in a case. It is a septimal balance, in which, as you will see, 1 lb. placed in the pan suspended from one of the extremities of the beam balances 7 lbs. placed in the pan suspended from a knife-edge placed one-seventh of the distance from the middle of the beam to the other extremity. It is of consequence to make the inspector's kit, which he has to carry with him, as light as possible. It contains a 4 lb. a 2 lb. and a 1 lb. weight, and a nest of small weights, weighing altogether 1 lb. more. By placing all these weights, amounting together to 8 lbs., in the pan suspended from the end of the beam, the inspector is enabled, as you may see, to test a 56 lb. weight placed in the other pan. This septimal balance has also the advantage of being a serviceable equal-armed balance, by having a second knife-edge at the extremity of the beam, from which a pan of equal weight to that at the other end can be suspended.

The other balance with unequal arms is a French balance, and is part of a similar kit supplied to the verifiers of weights and measures in France, where the decimal metric system is established. You see there is a knife-edge placed one-tenth of the distance from the middle of the beam to

one extremity, and when a weight of 10 kilograms is placed in the pan suspended from this knife-edge, it is balanced by a 1 kilogram weight in the pan suspended from the other end of the beam.

8. In both these kinds of balance the beams are levers of the first order, the fulcrum upon which the beam turns being placed between the power and the weight, that is to say, between those points of the beam which carry the testing power on one side and the tested weight on the other side. On the principle of the lever, the power of any weight to move a balance beam is proportionately greater according as the part of the beam which is pulled down by that weight is more distant from the fulcrum or the centre of motion of the balance. Hence it follows that the power of a weight to move a balance is in a ratio compounded of the weight itself and of the distance of its point of suspension from the centre of motion of the balance.

A multiplying or proportionate balance can consequently be constructed, as you have seen, for determining the weight of a body placed in the pan suspended from the shorter arm of the beam, by its being found equal to a multiple of a unit weight placed in the pan suspended from the longer arm of the beam, usually termed the *weight*-pan. Thus, if the beam be divided into say three equal parts, and the centre of motion be placed at the first of these divisions, one-third of the whole length of the beam, 1 lb. placed in the weight-pan will form an equipoise with 2 lbs. placed in the other pan, and so on.

9. The two balances with unequal arms, which have been exhibited to you, are specimens of this class of balance where there is a fixed proportion between the two arms. In these balances any weight placed in the weight-pan will equipose a proportionally larger weight in the other pan. But there are also balances of the same class where a similar result is produced by a variable proportion between the two arms, and by one fixed weight only, which is made to traverse along the graduated long arm of the balance. Such balances are, as you know, called by us steelyards. They appear to have been first used by the Romans, and to have been the earliest form of a well-constructed multiplying balance. In France they are still called Roman balances. You may here see a specimen of an ancient Roman steelyard, or *statera*, which has

been sent for exhibition, No. 332 in the catalogue. There is also a very fine specimen of a steelyard in the room below, exhibited by the Spanish Government, which could not conveniently be brought up here for you to see.

10. I may here shortly refer to some other balances of peculiar construction, which are exhibited in the collection. Mohr's balance (No. 4467 in the catalogue) is stated to be a balance with one arm. But you will see it is merely a balance with two unequal arms. There are also in the collection some hydrostatic balances intended for weighings in water. Hydrostatic weighings may, however, generally be made in an ordinary equal-armed balance of precision, by means of a short pan specially constructed for the purpose with a hook underneath, from which the body to be hydrostatically weighed is suspended by a very fine wire, and is immersed in a vessel of distilled water placed underneath the short pan.

11. I shall now go on to describe more particularly the principles of construction of equal-armed balances of precision, as balances for scientific weighings are usually called.

The chief requirements of such a balance are as follows:—

(I). The points of suspension of the pans from the beam ought to be exactly in the same line as the centre of motion, or the fulcrum on which the beam turns when set in action. The line joining these three points is the *axis* of the beam.

(II.) The two points of suspension of the pans should be exactly equidistant from the centre of motion. This is a *sine qua non* for a just equal-armed balance.

(III.) There should be a minimum of friction at the centre of motion and the two points of suspension of the pans.

(IV.) The centre of gravity of the beam should be placed a little below the centre of motion.

12. As to the effects of the relative positions of the centre of motion or fulcrum with the points of suspension and the centre of gravity of an equal-armed balance, loaded with equal weights, it is to be remarked:—

(*a*) When the centre of gravity *coincides* with the centre of motion of the beam, and the three knife-edges are in the same line, the beam of the balance will have no tendency to one position more than another, but will rest in any position in which it may be placed, whether the pans be suspended

to it or not, and whether the pans be empty or equally loaded. Such a balance is wanting in proper action.

(*b*) If the centre of gravity of the beam, when level, be immediately *above* the centre of motion, the beam will upset with the smallest action, that is to say the end which is lowest will descend, and it will descend with the greater velocity according as the centre of gravity is higher and the points of suspension of the pans less loaded.

(*c*) But if the centre of gravity of the beam be immediately *below* the fulcrum, the beam will not rest in any position but when level; and if disturbed from the level position it will vibrate, and at last come to rest in a horizontal position. Its vibrations will be quicker, and its tendency to the horizontal position stronger, the lower the centre of gravity and the less the weight upon the points of suspension of the pans.

Again, as to the relative position of the central knife-edge with the line joining the two outer knife-edges from which the pans are suspended, it is to be observed,

(1.) That if the fulcrum be *below* the line joining the points of suspension of the pans, and these be loaded, the beam will upset, unless prevented by the weight of the beam tending to produce a horizontal position, as shown in *c*. In such case a small weight will produce an equipoise; in the case of *a*, a certain exact weight will cause the beam to rest in any position, and all greater weights will cause the beam to upset, as in *b*.

(2.) If the centre of motion of the beam be *above* the line joining the points of suspension of the pans, the beam will come to its horizontal position, unless prevented by its own weight, as in *b*.

(3.) If the centre of motion of the beam coincide with the line joining the points of suspension of the pans, and very nearly with the centre of gravity, all the vibrations of the loaded beam will be in lines nearly equal, unless the weights be very small, when the vibrations will be slower. The higher the fulcrum the slower will be the vibrations of the balance, and the stronger the horizontal tendency.

It thus becomes evident that the nearer the centre of gravity is to the centre of motion, the more delicate will be the balance, and the slower its vibrations. The tendency to a horizontal position is therefore increased by lowering the

centre of gravity, in which case it will require an additional weight to cause it to turn or incline to any given angle, and it is therefore less sensible with a greater load. The fixing of the centre of motion in a balance is consequently of peculiar importance, for on this depends the readiness with which it will be affected by a smaller weight, and will return to a horizontal position. And it will be seen that the best position of all is that where the centre of motion is a little above the centre of gravity. It should, however, be proportioned to the distance of the weights from the fulcrum and to the amount of the load, and this object can be attained in different beams only by practical skill and experience. In order to regulate the centre of gravity in balances of precision, they are made to carry a small weight either under or over the centre of motion, and movable with a screw.

If the beam of an equal-armed balance be adjusted as to its centre of gravity so as to have no tendency to one position more than another, as in *a*, and the pans be equally loaded, then with a small weight added to one of the pans, the balance will turn, and the point of suspension of this pan will move with an accelerated motion similar to that of falling bodies, but very nearly as much slower in proportion as the added weight is less than the whole weight borne by the fulcrum. The stronger the tendency to a horizontal position in a balance, or the quicker its vibration (as in *c* and 3), the greater additional weight will be required to cause it to turn or oscillate to an increased angle. If a balance were to turn with $\frac{1}{10000}$th part of the weight upon the fulcrum, it would move at the quickest 10,000 slower than a falling body; that is to say, the pan containing the weight, instead of moving at the normal rate of a falling body, or through sixteen feet in a second of time, would fall only through about $\frac{1}{50}$th of an inch, and it would take about thirteen seconds to fall $\frac{1}{4}$ inch. Consequently all accurate weighing with a balance of precision which turns, with a very small difference in the weights placed in the pans, must be slow.

13. From what has been said it may be seen that if the arms of a balance be unequal, weights which form an equipoise will be unequal in proportion. A justly constructed equal-armed balance may thus be tampered with and made a fraudulent balance. One common practice with fraudulent dealers is to throw one of the suspending chains of the weight-

pan over the beam, and thus bring its point of suspension nearer the centre of motion. There are few of the poorer purchasers who either perceive or understand this act and its result. But you will see that if the beam be sixteen inches long and the chain is thrown over a point two inches nearer the centre of motion, the effect must be to diminish the weighing power of any weights placed in the pan by one-fourth, and to defraud the purchaser to this extent in the weight of the goods sold to him.

A similar fraud is sometimes practised by butchers, with equal-armed balances having a hook at each end to suspend the pans. On bending the hook from which the weights are suspended inwards, the weighing power of these weights is diminished with a result similar to that already shown; and again, by bending the other hook outwards, and thus lengthening this end of the lever, a piece of meat actually weighing 4 lbs. may be made apparently to weigh 5 lbs. In our collection at the Standards Office, we have a fraudulent beam of this description which was seized from a butcher by an inspector of weights and measures, and I have brought it here to show it to you.

I may also here mention another form of equal-armed balance, much used both in this country and abroad, where the pans are placed above the beam and knife-edges, which are jointed together. If these balances are not properly constructed they also open a wide gap to fraud. I could show you a balance of this description which has been deposited at the Standards Office, where a 1 lb. weight placed in the centre of the goods pan exactly counterpoises a 1 lb. weight in the weight pan. But if placed at the inside edge it is $1\frac{1}{4}$ oz. against the purchaser, whilst if placed on the outside edge it is in favour of a buyer $1\frac{1}{4}$ oz. A dealer may thus gain $2\frac{1}{2}$ oz. in every 16 ozs. in the quantity of a commodity that he buys and sells again, though weighed in the same balance and with the same weights.

14. In further illustration of these principles of construction of a good equal-armed balance, the actual construction of the several parts of the balance will now be explained. The fulcrum on which the beam of the balance rests and turns at its centre of motion is a horizontal plane of polished steel or agate supported on the column of the balance. The beam rests upon this fulcrum or bearing by means of a knife-edge

of hardened and polished steel; the line of the knife-edge is also in a horizontal plane at right angles to the axis of the beam. The whole length of the knife-edge ought consequently to be in contact with the plane of the fulcrum at every point. This knife-edge is at the lower edge of a steel prism, the section of which is an equilateral triangle; and the edge is ground with a small facet on each side so as to increase the angle at the edge from 60° to 120°. Captain Kater, who was an eminent member of the Royal Society fifty years ago, and took the most active part, as a member of the Standards Commission, in superintending the construction of the imperial standard weights and measures which were legalised in 1825, considered the angle of 120° to be practically the best form for the knife-edge of a balance of precision, and the balances of the Standards Department have since been adjusted to this angle. It requires great care in a skilled workman to adjust the knife-edges, as the excellence of the action of balances depends very much upon them. The small facets on each side of the knife-edge ought to be exactly rectangular.

Whilst the central knife-edge is at the lowest part of the prism, the two knife-edges at the ends of the beam have, on the contrary, their edges uppermost, and you will see that the tendency of this arrangement is to bring the centre of gravity of the balance a little below the centre of motion. The pans are suspended from these two knife-edges by means of agate or steel planes bearing upon them.

In order to preserve the nice adjustment of the knife-edges, they are never allowed to be in contact with their bearings, except when weighings are actually being made. At all other times the beam and pans are supported upon a frame attached to the column of the balance, and movable in a vertical direction upon it. When the balance is required to be put in action, the support is very gradually lowered by means of a lever handle, and the knife-edges are brought upon their bearings, so that the balance is left free to act. As soon as the weighing is completed the supporting frame is again raised, and the knife-edges are thus lifted from their bearings.

15. The chief cause of discordances in the results of successive series of weighings with a balance of precision, which it is necessary to guard against, is the risk of the knife-edges not being brought again to exactly the same place on their

plane bearings after the balance has been stopped and again set in action. The most perfect balance is one that varies

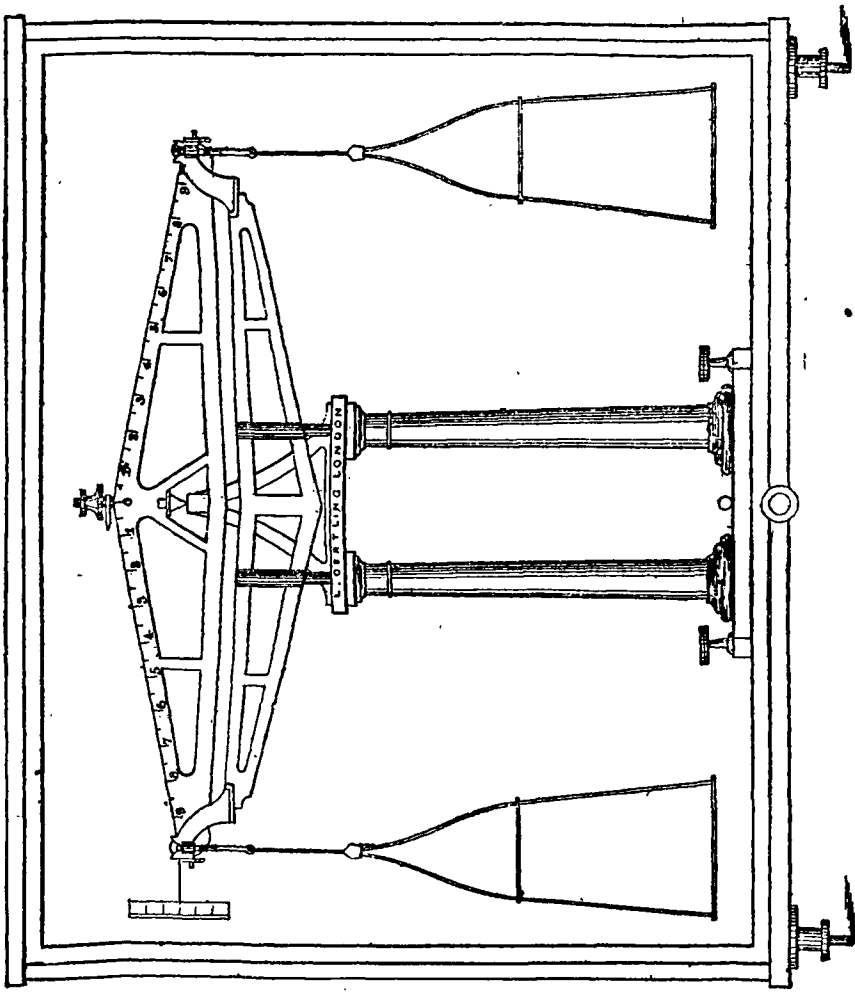


FIG. 1.—Kilogram Balance of Standards Department.

least in the points of contact between the knife-edges and their bearings during a successive series of weighings. For

the attainment of this important object, the supporting frame is furnished at each of its extremities with two steel pins terminating in cones, and made to fit exactly into corresponding conical holes in the plane bearings over the knife-edges at the ends of the beam. These two conical holes are in a line with, and on each side of the knife-edge. The lines joining the points of the two pins and of the conical holes are thus in a line normal to the axis of the beam, and all the points are in the same horizontal plane when the balance is at rest. As the movement of the supporting frame in a well-constructed balance of precision is always in the same vertical line, being guided by vertical rods fitted to cylindrical drilled holes in the column of the balance, the knife-edges and their bearings are thus always brought into contact in the same relative positions.

The arrangements of the knife-edges and of the supporting frame may be seen in the kilogram balance constructed by Oertling, now exhibited and illustrated by a large drawing. (See Fig. 1.) To show the great importance of avoiding every risk of a displacement of the knife-edges on their bearings by the most exact agreement being maintained between the position of the supporting frame and of the beam itself, it should be pointed out that in this balance the conical pins are a fixed part of the supporting frame, and must descend and ascend in the same vertical line, whilst the points of the conical holes, being movable with the beam, must move in the line of the circumference of a circle, the radius of which is the length of the axial line from the centre of motion of the balance to these points. There is consequently a risk of displacement from any such difference in the line of motion of the pins and their conical holes, although the risk is diminished in proportion to the length of the beam. Such risk did not escape the observation of Captain Kater, and in some balances made under his direction by Mr. Bate an arrangement was made by means of two side rods hinged to the central vertical rod, and of the proper radial length, to give the same circular motion to the supporting pins as the conical holes. You may see a similar contrivance in the small Mendeleef balance now before you, which is exhibited by me, No. 344*a* in the catalogue. (See Fig. 2.)

16. Attention should here be called to the difference of construction of these two balances of precision, each of them

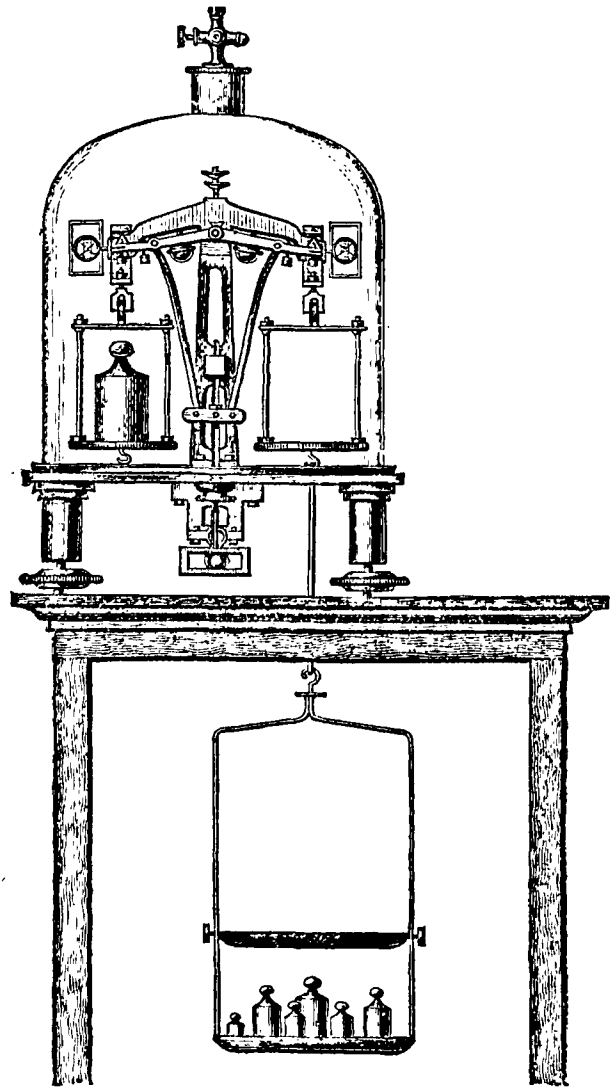


Fig. 2.—Mendeleef Balance.

constructed by Mr. Oertling, and made to carry a kilogram in each pan, but differing materially in the length of their beams.

Long beams have generally been recommended for balances of precision, because the quantity of motion in any point of a lever varies as its distance from the fulcrum; and, therefore, the greater the distance of the parts of suspension from the centre of motion of an equal-armed balance, the more distinguishable will be the motion arising from any small difference between the weights compared. On the other hand, there are certain advantages in the quicker angular motion, greater strength, and less weight of a short beam. The larger balance has a beam of ordinary length, being 20 inches long and about 2 lbs. in weight. Its sensibility is so great when in proper condition, that, loaded with a kilogram in each pan, a milligram (equal to $\frac{15}{1000}$ grain, and which is $\frac{1}{1000000}$th part of a kilogram) added to one of the pans causes a difference in the resting point of the balance of about ten divisions of the index scale. One division, therefore, corresponds with 0·1 milligram.

17. The other balance has been quite recently constructed from a design by M. Mendeleef, Professor at the University of St. Petersburg. The length of the beam of this balance is only $4\frac{3}{4}$ inches, and its weight about $\frac{1}{4}$ lb. This balance has not yet been sufficiently tested as to its sensibility, but M. Mendeleef claims for his own balance, which served as a model for this balance, that one division of the scale is equivalent to 0·07 milligram only.

An attempt is here made, and, as it appears, not unsuccessfully, to overcome the disadvantage hitherto considered to be inherent in a short-armed balance: of its oscillations being necessarily of very small extent, and minute differences in weights compared being consequently not observable. The mode adopted is, as you may see, to employ a microscope with a micrometer for observing the smallest movement of the pointer of the beam over a very finely graduated index. The result has been that, with a kilogram in each pan, an additional weight of a milligram to one of the pans has been found to cause an observed difference of fifteen divisions of the scale. This mode of observing the oscillations of a balance is, however, not new; it was employed long ago, as I shall presently show. A fuller description of the Mendeleef

balance is contained in Appendix X. to the Ninth Annual Report of the Warden of the Standards.

The principal objection that first occurs to this method is that, as it requires the observer to stand close to one side of the balance, there is a tendency to the temperature of the beam becoming unequally affected by the greater heat of the observer's body, and thus to cause discordances in the results of successive weighings. But, on the other hand, the shorter the beam, the less risk there is of its being so acted on.

The mode of observing the oscillations of the longer-armed balance is not open to this objection. The observer is seated about five feet from the balance case, and views the movement of the pointer over the index through a telescope. Any difference of one-tenth of a division is thus readily observed by estimation.

One great advantage of this short-armed balance is that, being so small, and placed as it is upon a metallic plate, it can be covered with an ordinary glass bell receiver, and used as a vacuum balance. In weighings in a vacuum it is very important to be able to exhaust the air rapidly and frequently; and that the balance-case serving as a receiver should be as small as possible, so as to reduce to a minimum the volume of air to be exhausted.

18. The pans of a balance should be suspended in such a manner that in all positions the corresponding rods or chains of the two pans may be parallel to one another; else the weights, though equal, will not be in equilibrium.

In weighings with balances of precision, the degree of preponderance of either pan is indicated by a needle or pointer fixed to the beam, either at its centre, in a line perpendicular to the axis of the beam and pointing downwards, or at either end and in continuation of its axis. In both cases the pointer moves along a graduated scale. But an index pointer placed perpendicularly to the beam affects its equilibrium when turned from its horizontal position; the measure of the momentum of the pointer being its weight multiplied by the distance of its centre of gravity from the vertical line. The error thence arising may, however, be corrected by continuing the pointer, or counterpoising it, on the opposite side of the beam.

The finest balances of the Standards Department have the index pointer at each end of the beam, as shown in Fig. 1.

For all weighings requiring special accuracy, the highest and lowest points reached by the needle in successive oscillations of the balance are read on the index scale. In each case the mean between the highest and lowest readings is noted as the resting point of the balance.

Balances of precision are always enclosed in glass cases, with a view both to their preservation, and more especially to keep their action in weighings as far as possible from being influenced by draughts of air and alternations of temperature, which would affect the accuracy of the results.

19. Amongst the balances of the Standards Department there is, however, one balance of peculiar construction and of extreme delicacy, to which I should now call your attention. It was used by Professor Miller for all his weighings during the construction of the new Imperial Standard pound, including his weighings of a kilogram. It has been lent to the International Metric Commission at Paris for the weighings of the new kilograms, or it would have been sent here for exhibition as a scientific balance of considerable historical interest. This balance was constructed by Barrow, and is similar in construction to Robinson's balances. The distance between the extreme knife-edges is 15·06 inches. The knife-edges work upon quartz planes. The middle knife-edge is 1·93 inch long. Index scales marked upon thin and nearly transparent slips of ivory, a little more than half-an-inch long, are fixed to each side of the beam and oscillate with it. There are 50 divisions of the scale, 0·01 inch apart. The scale is viewed through a compound microscope, fixed in the glass case of the balance, and having a single horizontal cobweb in the focus of the eyepiece. A glass screen is interposed between the observer and the front of the balance case. The mean value of one division of the scale was found to be about 0·002 grain with 1 lb. in each pan, and 0·005 grain with a kilogram in each pan. All the results of his comparisons were noted by Prof. Miller in hundredths of a division of the scale.

20. In describing the mode of observing the oscillations of a balance of precision, I should not omit also to call attention to the excellent method used by the late Dr. Steinheil of Munich, through the adoption of a principle originated by Gauss. The movements of the beam are observed by means of a small mirror placed immediately over the central knife-

edge, and with its plane surface normal to the axis of the beam. The observer is placed about 12 feet distant from the mirror, and views through a telescope the reflection in the mirror of a vertical graduated scale placed close to the telescope. The ray of light reflected from the mirror and thrown upon the graduated scale serves to indicate with great exactness by its angular deviation the difference of the standard weights compared. The amount of difference is ascertained by reading off the reflected scale the highest and lowest numbers at the turning-points of the balance coinciding with a horizontal thread fixed in the telescope.

21. As balances of precision are alone used for scientific weighings, it is not proposed now to enter upon the subject of the larger kind of multiplying balances, such as platform weighing machines, where a unit weight is multiplied one hundred fold, as these are used for commercial purposes only. I may merely direct your attention to this model of a centesimal balance, exhibited No. 382, in which the only principle involved is the most convenient mode of extending the length of the longer arm of the lever. It is, however, worth your attention as a practical illustration of the mechanical power of the lever.

22. The construction of balances of precision with knife-edges on agate or hard steel plane bearings, with contrivances for keeping them in position, is comparatively of recent date. The best balances were previously constructed with knife-edges on curved bearings enclosed in steel boxes to prevent shifting. Curved bearings only are generally used for commercial balances.

23. You see here two balances of precision of peculiar construction. The first is a balance made by Troughton early in the present century, and formerly belonged to Sir James South. It has recently been purchased for the Standards Department. The beam is of wood, and it vibrates upon two steel points resting on an agate plate. The pans are suspended from pins placed at equal distances from the middle of the line joining the two points which form the centre of motion, by means of thin cords passing over the ends of the beam. I propose to leave this balance to be seen in the Loan Exhibition.

24. The second is a balance recently made for the Standards Department by Mr. Oertling from a design by Mr. Artingstall,

No. 178 in the Catalogue. Its peculiarity is that thin elastic steel springs are used both for the centre of motion of the balance and for suspending the pans. It is similar in principle to a balance constructed by the late Dr. Steinheil of Munich, in which silk ribbon was used instead of steel springs. Such balances possess the advantages of simplicity of construction and durability. But they are wanting in the sensibility and stability requisite for a good scientific balance.

25. I ought not to omit to mention also the beautifully constructed automatic balances for testing with great accuracy the weight of gold coins, invented by Mr. Napier, and now used at the Bank of England and the Mints of many countries. Once set in motion, these balances continue to test the weight of sovereigns placed successively in one of the pans. If correct, the sovereign is thrust aside into a central drawer; if heavy, into a drawer on one side; if light, into a drawer at the other side. A single man, or even a small boy only, is required to keep an upright cylinder supplied with coins to be tested.

A most ingenious improvement upon these automatic balances has since been made by Mr. Napier. He has added a contrivance by which every heavy sovereign is shifted to a second pan, where its weight is reduced to the correct weight; and then it also is thrust aside into the central drawer.

26. Having thus fully described the construction of balances of precision, I come to the scientific methods of accurate weighing.

The ordinary mode of commercial weighing, by putting the commodity to be weighed in one scale and weights in the other until an equilibirum is attained, is insufficient for scientific weighings, as the results are subject to errors arising from defects in the balance itself. To avoid any such errors and attain scientific precision in the results, a check is required, which is found in a system of double weighing, and in taking the mean result of a series of successive weighings.

27. Two methods of double weighing are commonly used. One method, known as Borda's and generally used in France, is that of *substitution*, or weighing separately two bodies to be compared against a counterpoise, by placing them successively in the same pan, and thus ascertaining their difference in divisions of the index. The second method, known as

Gauss's, but which was first invented by Le Père Amyot, and is now more generally used in England and Germany, is that of *alternation*, or first weighing the two bodies against each other, and then repeating the weighings after interchanging the weights in the pans. By this second method, no counterpoise is required, and *half* the difference between the two mean resting-points of the index pointer of the balance shows the difference of the weight of the two bodies in divisions of the scale.

As the value of a division is continually liable to variation according to the condition of the balance, the state of the atmosphere, the weight in the pans, &c., it is necessary, for attaining very accurate results, to determine the value of a division for each comparison. This is done by an additional weighing after a very small balance weight, the value of which is exactly known, has been added to one of the pans, so that its effect on the reading of the index scale may be ascertained.

28. The most accurate mode generally adopted for noting the results of the oscillations of the balance by observing the movements of the pointer over the index scale, is as follows. We will first take it as illustrating Gauss's method:

In weighing two bodies A and B against each other, place A in the left-hand pan x, and B in the right-hand pan y, and set the balance in motion. The telescope should be previously adjusted to the index scale on the left-hand side of the balance, so as to enable the observer to see the effect of the weight of A against B. The first turn of the pointer is always to be disregarded, and the readings of the index scale at the next three turns are to be noted. Then stop the balance. The reading at the third turn of the pointer, and the mean of the two readings at the second and fourth turns, are taken as the extreme readings, or the highest and lowest, and their mean is the resting point of the balance. This constitutes one *observation*.

The second observation, which completes one *comparison*, is made by interchanging the two bodies A and B, by moving each to the other side of the balance, when similar readings are taken of the weight of B against A. As before stated, half the difference between the two resting points of the balance shows the difference of weight of the two bodies A and B in divisions of the scale. An additional weighing is

then taken, after adding a small balance weight to either pan, in order to ascertain the value of a division.

In cases where great accuracy is required, any number of successive comparisons may be made in like manner with the object of taking the mean result of them all. This course not only lessens the probable error of the result, but is also a check against any accidental mistake, either in noting the readings or in the computations. It is important to make the weighing in as short a time as possible, so as to avoid the risk of discordances in the results arising from variations of temperature, of moisture in the air, or other causes; and it is better to take only two or three comparisons at a time, and to repeat them on subsequent days, taking the mean result of all the comparisons.

29. In weighing by Gauss's method, it is very desirable to be able to interchange the weights from one side of the balance to the other without opening the balance case, and thus to avoid the risk of changes of temperature of the air of the balance case, and consequent production of currents of air. For this purpose the plan is adopted of interchanging the pans as well as the weights, and it not only gives the advantage of avoiding the risk of injury to the weights by taking them up in the ordinary way with a pair of tongs, but is especially useful when either of the bodies weighed consists of several separate weights. The pans are readily interchanged by lifting each of them from its place at the end of the beam by means of a brass rod with a curved end introduced through a hole in the side of the balance case, and transferring it to a hook suspended from a brass slider made to move over and parallel to the beam. Thus each pan with its load is slid over and transferred to the other side of the balance.

30. You will better understand the mode of interchanging the weights and pans used in Gauss's method of weighing by a practical illustration.

At the Standards Office we use printed forms for noting and recording the weighings, one of which I have brought you, filled up with the actual results of one of the comparisons of the platinum-iridium lb., designated as P-i., which I have already exhibited to you, with the Imperial Standard lb. of platinum, designated as PS. The following shows the mean results of five observations:—

First Comparison of Platinum-iridium 1 *lb.* (*P-i*), *with the Platinum Imperial Standard lb.* (*PS*); 21 *July*, 1874.

Compared.	Comparing.	Readings of Index Scale in Divisions.						Balance No. 3, Value of 1 division.	Compared = Comparing.	
		Of compared.			Of comparing.				In divisions.	In grains.
		Highest.	Lowest.	Mean.	Highest.	Lowest.	Mean.			
P-i + y	PS + x	(1) 56	15	35·5	(2) 30·3	62	46·15	Gr. 0·002	−5·2875	−0·01057
P-i + x	PS + y	(3) 63	10	36·5	(4) 50·0	44	47·00			
				36·0			46·575	Difference = 2)10·575 5·2875 0·002 0·0105750		
t = 19°·62 C. b = 755·09 mm. at 0° C.		to find value of 1 div. Gr. 0·01 added to (3)			(5) 50	13	31·5	(3) − (5) = 5)0·01 0·002		

∴ P-i = PS − 0·01057 gr. in air, t = 19°·62, b = 755·09 mm. at 0° C.

A similar mode of noting the weighings is pursued in a comparison by Borda's method, when for the two observations a counterpoise weight remains in the right-hand pan *y*, and the two bodies A and B are weighed successively against this counterpoise, the difference between A and B being shown in divisions of the index scale by the whole difference between the two resting points of the balance.

31. It has been mathematically proved that Gauss's method gives twice as accurate a result of a single observation as Borda's method. In fact, as the distance between the two positions of equilibrium, or resting points of the balance, serves to measure the difference between the two weights compared, and this distance is by Gauss's method reduced to one half less than by Borda's method, it is evident that the result of a single observation by Gauss's method is twice as accurate as by Borda's, and therefore, that one comparison by Gauss's method gives as good a result as four comparisons by Borda's method. In other words, the probable error of the result of a comparison by Borda's method is four times as great as by Gauss's method.

32. The methods of weighings of which I have spoken give only the apparent difference of weight in air of the two bodies compared. In order to ascertain their true difference in weight it is necessary to allow for the weight of air displaced by each,—in other words, to reduce the weighings to a vacuum.

The formula for this purpose is as follows:—

If the weights A and B appear to be equal when weighed in air, then the weight of A − the weight of air displaced by A = weight of B − weight of air displaced by B.

The justness of this mode of correction is obvious from the following considerations:—

A body weighed in water weighs less than when weighed in air by the difference between the weight of water and weight of air displaced by its volume. In like manner a body weighed in air weighs less than when weighed in a vacuum by the weight of air which its volume displaces. It follows that, for instance, a brass weight, if it weighs exactly one pound in a vacuum, must when weighed in air weigh less than a pound by the weight of air that it displaces, and its true weight, when weighed in air, must consequently be found by deducting the weight of air thus displaced.

33. But the weight of a given volume of air is necessarily greater or less according to the pressure of the atmosphere, the temperature of the air and other conditions affecting it.

In comparing two standard weights, the weight of air displaced by each is computed from the following data :—

As regards the weight of air,

(1.) The observed mean barometric pressure during the comparison, reduced to 32° F. and corrected by deducting the pressure of vapour and of carbonic acid gas in the air.

(2.) The mean temperature of the air.

As regards the volume of the standard weight displacing the air,

(3.) The density of each weight.

(4.) Their mean temperature and coefficient of cubic expansion.

(5.) The actual weight of each standard.

34. From data 1 and 2 the ratio of the density of the air to the maximum density of water must be ascertained. This ratio is also affected by the latitude of the place where the comparisons are made, and its height in relation to the mean level of the sea, as the force of gravity differs accordingly, as I have before explained to you. For determining this ratio, tables have been prepared by Professor Miller, and are used at the Standards Office. They are printed in Appendix V. to the Fifth Report of the Standards Commission. These tables are also available at other places by making a slight correction for difference of latitude and height in relation to sea level. They are based upon the weight of a litre of dry atmospheric air as deduced from M. Regnault's observations at Paris, viz. : 1·2932227 gramme at 0° C., in latitude 48° 50′ 14″, 60 metres above sea-level, with a barometric pressure of 760 millimetres of mercury ; and assuming that atmospheric air contains on an average 0·0004 of its volume of carbonic acid, and that the pressure of vapour in comparing rooms is on an average two-thirds of the maximum pressure due to the temperature, the density of the vapour of water being 0·622 of the density of air.

35. As regards the other data, the first to be considered is the density of each weight.

The *density* of a body is defined as the *mass* contained in its volume when referred to a uniform standard. Its density

is to be distinguished from its *specific gravity*, which shows its weight in relation to its volume, also when referred to a uniform standard. In shorter terms,—the density of a body is the quantity of matter in a unit of its volume; the specific gravity of a body is the weight of a unit of its volume. The relation of the bulk or volume of a body to its weight is thus expressed, both as to its density and its specific gravity, and these terms are often used indiscriminately. But the former term is more strictly applicable to solid bodies, and specific gravity to liquids and gases. The densities of bodies are in the direct ratio of their masses, and the inverse ratio of their volumes.

The unit of density which is adopted is the volume of an equal bulk of distilled water at its maximum density, that is to say at the temperature of about 4° C., or 39° Fahr. The numerical value of the density of a body is usually obtained by hydrostatic weighing, or weighing it in distilled water and comparing the weight of water displaced by it with its own ascertained weight in a vacuum. When no temperature is stated, it is generally understood that the numerical value assigned is that at 0° C., or 32° Fahr.

The density of a body may also be determined from its cubic measurement in relation to its weight as compared with a weight of known density, where the form of the body admits of the measurement being accurately made. But this is seldom practicable.

As to No. 4 of the data, it is evident that the density of each weight varies according to its temperature and rate of expansion, and that these influences must be allowed for in determining its mean volume, in order to ascertain the volume of air displaced by it during the comparisons.

36. The actual mode of computing the weight in grains of air displaced by a standard weight is by adding the logarithms of the following terms:—

(1.) Of the barometric pressure in millimetres, deducting the pressure of vapour in the air, in millimetres of mercury.

(2.) Of the ratio of density of the air at the observed Centigrade temperature to the maximum density of water.

(3.) Of the ratio of the density of the weight at 0° C. to its density at the observed temperature and its ascertained rate of expansion.

(4.) Of the weight of the standard in grains.

And by deducting from the sum the logarithm

(5.) Of the ratio of the density of the standard at 0° C. to the maximum density of water.

The logarithms of terms 1, 2, and 4 are readily obtained from the printed tables used by the Standards Department, which are those used by Professor Miller, and described by him in his account of the construction of the imperial standard pound in *Phil. Trans.* 1856, Part III. p. 784.

Of course, in comparing two standard weights of equal density, or where no special scientific accuracy is required, their apparent relative weight in air may be taken as their true relative weight. This, however, will not give their true or absolute weight, which is their weight in a vacuum, unless the exact weight of air displaced by them is ascertained.

37. The time allotted to this lecture does not allow me to explain more fully the mode of computing the weight of air displaced by a standard weight. But a sufficient idea of the effect of the difference of density in standard weights composed of different materials may be given from the following table of the weight of air displaced by 1 lb. avoirdupois weights differing in density. It may be seen that the weight of air displaced is in an inverse ratio to the density:—

Table of the Weight of Standard Air ($t = 62°$ F., $b = 30$ inches) displaced by 1 lb. avoirdupois weights differing in density:—

Description of 1 lb. weights.	Density at 0° C.	Weight of air displaced.
Imperial Standard—Platinum	21·157	0·403 gr.
Copy of ditto Platinum-iridium .	21·425	0·398
Ditto Gilt Bronze. . . .	8·283	1·029
Commercial Standard of Brass	8·143	1·047
Ordinary Cast Iron	7·127	1·177
Copy of Imperial Standard—Quartz . .	2·650	3·217
Ditto Glass. . .	2·505	3·385

38. You may now see a practical illustration of the effect of the difference of density in weights. Here are three 1 lb.

avoirdupois iron weights, of the same apparent bulk. But one only is a just weight. The second and third are fraudulent weights seized from itinerant dealers; they have had their insides bored out, and filled with matter of less density,—one with cork blackened at the bottom, the other containing only air. The result is that the first weighs exactly 1 lb. or 16 oz., the second $7\frac{3}{4}$ oz., and the third $7\frac{1}{4}$ oz.

39. The work of computing the weight of air displaced by standard weights compared is to be avoided by weighing them in a balance placed in a vacuum. The vacuum balance of the Standards is a large and costly instrument of high scientific character, and could not well be moved to this Loan Exhibition. I have here a drawing of it and a full description as shown in Appendix VIII. to my Seventh Annual Report. But the small Mendeleef balance is, as I have already stated, intended to be made available as a vacuum balance; and although it is a new and untried instrument, we have endeavoured to get it sufficiently ready to show you the action of a vacuum balance. The bell-shaped cover of the balance, which you now see, is merely a temporary one. It is far too large, and it is proposed to have one of a different form, and made as close as possible to the balance.

40. A further practical illustration of the effect of the density of a weight when compared with another weight of different density will now be exhibited to you, by comparing first in air and then in a vacuum, or something near it, the platinum-iridium lb. P-i, with the quartz standard lb. of the Standards Department, designated as Q.

P-i was intended to be of the true weight of 1 lb. *in a vacuum.* Its actual weight in a vacuum, as deduced from the mean result of all the comparisons with PS, has been determined to be 0·01691 gr. in deficiency, and, as already shown, it displaces nearly 0·4 gr. of standard air. Its apparent weight in air is therefore about 0·4 gr. *less* than its weight in a vacuum.

Q was constructed to be as nearly as possible of the weight of a brass lb. *in air.* The commercial standard lb. of brass, which is a theoretical lb. of the average density of brass, displaces a little more than 1 gr. of standard air, whilst Q displaces a little more than 3·2 gr. Q was therefore constructed to be in a vacuum about 2·2 gr. heavier

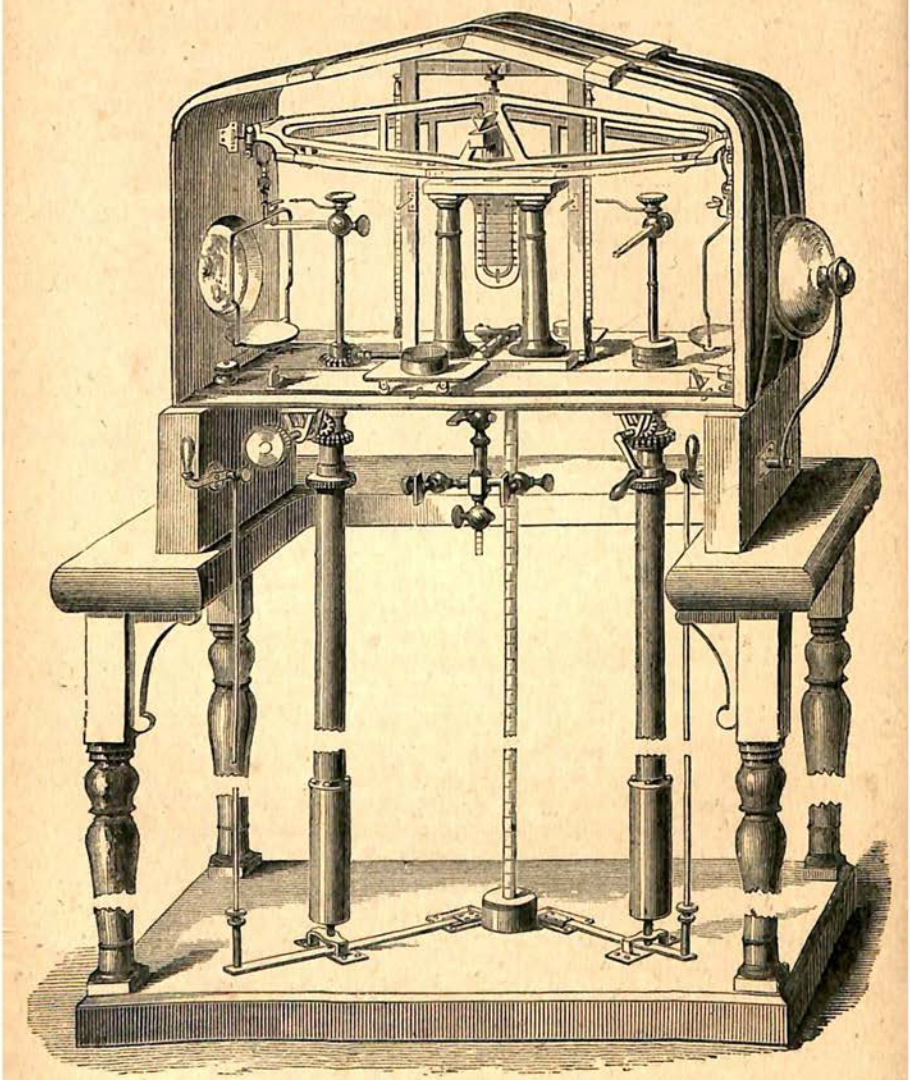


FIG. 3.—Vacuum Balance of Standards Department.

than the imperial standard lb. It is actually 2·368 gr. in excess. Its apparent weight in air is therefore about 0·8 gr. (3·217 — 2·368) less than 1 lb., whilst that of P-i is 0·4 gr. less.

To produce an equilibrium in air a weight of about 0·4 gr. should then be added to Q; and if, after the weighing in air, this added weight is left in the pan with Q, a weight of a little less than 2·8 gr. (2·368 + 0·4) should be added to P-i, in order to produce an equilibrium in a vacuum.

You may now see these two standard lbs. compared in air in the Mendeleef balance, and it takes nearly four-tenths of a grain, actually 0·35 gr. added to the quartz lb. to produce an equilibrium.

We will next exhaust the air from the receiver in which the balance is placed, so far as our means will readily allow, to show you the effect of withdrawing the air upon the difference of the two weights. If a perfect vacuum could be produced, and the thermometer and barometer had previously stood at the normal heights, it would be necessary, as I have stated, to add about 2·8 gr. to the platinum-iridium lb. to produce an equilibrium. But of course any difference from the normal temperature and barometric pressure must cause a slight variation in this amount of difference, as you have seen to have been the case in the weighing in air.

Before exhausting the air, we will then add somewhat less than 2·8 gr. to the platinum-iridium lb., in order that an equilibrium may be produced without going so far as an actual vacuum. There is a vacuum gauge in connection with the air-pump which shows a range of 4 inches of atmospheric pressure, and by adding a weight of 2·5 gr. we ought to produce an equilibrium with a pressure of about 4 inches of air. (*This result was shown.*)

41. In conclusion, I may mention to you that our collection of balances at the Standards Office, 7, Old Palace Yard, is well worth a visit. In particular, there is the large balance which was constructed for Captain Kater for verifying the imperial bushel by weighing its contents of distilled water, and which has been reconstructed by Mr. Oertling. The beam is of mahogany, 70 in. long, 2½ in. thick, and 20 in. deep in the middle. The central knife-edge is placed at half

of the depth. The whole balance is inclosed in a case of plate glass.

There is also the large vacuum balance, constructed by Mr. Oertling from a design of Professor Miller, with contrivances for changing the pans and weights, and putting in or taking out small balance-weights from either of the pans; together with a large collection of other scientific instruments, all of which will be readily exhibited to you at any time that may be appointed.

GEOMETRICAL AND ENGINEERING DRAWING.

BY PROFESSOR T. F. PIGOT.

In the two lectures which I am about to deliver, I shall confine myself as much as possible to an explanation of the apparatus contained in this collection. In the teaching of geometrical drawing, large recourse should be had to models, and there is a fine collection in this exhibition, which will be, in part, the subject of the present lecture. The knowledge required by civil engineers for drawing diagrams and designs of various kinds is of great extent, embracing the many distinct branches into which civil engineering is divided. Of these I may enumerate canals, rivers, roads, railways, bridge construction (including strength of materials), hydraulic engineering, harbours, lighthouses, and finally, surveying, which is required for all these purposes. This last, I understand, will be dealt with in another lecture, so that I have no further reference to make to it.

It so happens that this exhibition, large as it is, is singularly deficient in good models connected with the subjects I have enumerated, except in the one case of lighthouses; and I have, therefore, selected the subject of lighthouse illumination for my second lecture. It is true that there are, scattered up and down through this enormous collection, very many objects bearing on engineering, such, for instance, as Mr. Clark's hydraulic canal lift, which I believe is here; a fine collection of rain-gauges, wind-gauges, and current meters, besides a vast number of drawings and models of steam-engines, and other machinery.

But on no subject of civil engineering is there any complete series of models except the one I have alluded to.

I shall now call your attention to the collection of drawing instruments, which are, as you see, principally of the ordinary mechanical construction; but there are a few other instruments which I shall briefly explain. Before I do so, I think I should remind you—especially those of you who are students in drawing—of the absolute necessity for good instruments. Without them it is impossible to acquire real neatness of execution; and I have generally found that the use of inferior instruments leads beginners, at least into habits of slovenly execution, and in all cases greatly retards their advancement. Among the cases of ordinary instruments I have selected some boxes of English and some of foreign makers. These which I have before me are exhibited by Mr. Stanley, and with most of them you are familiar. Among the foreign makers Tacher has sent a very good collection. Besides these instruments in cases there are a variety for special purposes. Here are, on a board, some of Stanley's protractors. First there is the ordinary protractor, which, as you know, is used for setting off angles in all kinds of drawing, more especially for surveying, where it is necessary that these angles should be very carefully measured. Here is one of a peculiar kind, with three arms, commonly called a station pointer, for fixing the position of a point from three given points. Two of the arms are fitted with verniers for obtaining very accurate angles. In delicate work, such as laying down a long base line, a protractor is not sufficiently accurate, and recourse is had to other methods of laying down angles, as for instance setting out tangents with the assistance of a table of logarithms. There are some other special instruments, such as the pantograph, the eidograph, the elliptograph, the conchoidograph, and helicograph, an instrument for drawing spirals. This latter is a very elegant instrument, and might be used for drawing Ionic columns. There is a little wheel in the centre, which may be set to any required degree of obliquity, and when thus slightly turned to one side it forces round by a rack motion the whole instrument, which is only fixed at one point, and gradually the tracing point recedes from the centre, describing the spiral.

Lastly, I may mention another very beautiful instrument, although one not of much practical use for engineering purposes. It is what is called the geometrical pen. There are a great variety of figures on the board on which it is placed, which can be traced by it; but they are all nothing more than either circles, cycloids, epicycloids, or the combination of one with the other; the whole apparatus depends on these three wheels, which are convertible. According as you shift this point and as you change the size of the wheels, of which you see a great number here, you can produce any of these curves you choose.

Now, although I have mentioned these instruments, every draughtsman should be familiar with the curves which they are arranged to draw, so as to be able to draw them at points of his work where it would be utterly impossible to use these geometrical instruments, and in any case no instrument, however perfect, will supply a knowledge of freehand drawing; for almost every architectural or engineering drawing contains some portions which cannot be executed with the compasses or with ordinary drawing pens or instruments, and for which recourse must be had to the common pen. There are two instruments with which I have no doubt you are familiar —some of the oldest and best known, and which are really of great use. They are both intended for enlarging or diminishing drawings. One is called the eidograph, and its intention is to enlarge or copy plans. You see here two plans, one large and one small, the small one being diminished from the original by the eidograph. This consists of two bars, which are rigidly parallel, retained in their places by two steel bands. At this point there is the instrument for tracing, and here is the point round which the whole instrument can rotate. Here is a rod which simply keeps the instrument in its place: and at the other end is the pointer or tracer. The stylet, fulcrum, and tracing-point are all in a straight line, and the distances of the tracing-point and stylet from the fulcrum are in the ratio you wish to have your drawing reduced or enlarged. Then here is another instrument, called the pantograph, of which there are several in the exhibition, and which would serve the same purpose as the eidograph.

No doubt you are familiar with its principle, which is described in every book treating of drawing.

There is one pantograph down stairs to which I would call your attention. It is fixed upon a vertical pillar, and under it is a table upon which it moves. It is intended for etching and engraving.

I had intended explaining another instrument, the planimeter, which is used in surveying, but as there will be a special lecture on that subject, I have no doubt it will be fully explained to you then.

Before commencing to describe the geometrical models I shall say a word about geometrical drawing and descriptive geometry, in order to impress upon you their use, and almost their necessity, for engineering purposes. I need not tell those who are present, that draughtsmen are daily becoming more and more wanted, and that it is, besides, daily easier to obtain the services of foreigners; and thus it becomes the more essential that, we, in these countries, should learn earlier the subjects that boys abroad, especially in Germany, are perfectly familiar with. Descriptive geometry, which may be called the theoretical part of drawing, enables us to draw plane figures, surfaces, and solids with their intersections, many of which would be very difficult to represent without its aid. Besides there are its various applications, of which I may enumerate, first, what is called by the French *plans cotés*, or in English "figured heights," which is the representation of solids on one plane only, the figured heights which are marked on the plan showing the elevation of the various parts. The most important part of this subject is what we call *Contouring*, which you are no doubt familiar with. By this means, using what are called "figured heights" on the plan, you can delineate with accuracy any particular section of a country. The next application I should mention is that of shadows, or shading, and next comes perspective.

Besides there is a subject which we rarely treat of here—what is called gnomonics, or the construction of sun-dials. It was long considered of great importance, and is still largely used on the Continent.

Then comes stone-cutting, by which the true surfaces to which the stones are to be worked for architectural purposes are determined, and templates constructed for

the workmen. Then carpentry, which includes the accurate delineation of joints, mortices, wooden staircases, roofs, &c. Last of all I may mention isometric projection, although, in truth, that is nothing more than an example of descriptive geometry. It is, however, quite possible to learn stone-cutting, and one or two other branches, without knowing anything of descriptive geometry. If all these various branches of drawing are thoroughly mastered, it becomes very easy to apply them to any engineering designs. However complicated, their execution is then a mere matter of time and practice, though without some fundamental notion of descriptive geometry, draughtsmen have often to struggle against difficulties of execution, which they could have resolved without any trouble by a little previous study of this most useful subject.

I shall now proceed briefly to explain a few of the models and instruments before us. There is on the wall a series of diagrams, or rather a small number of the series, which I shall briefly explain. They were published by the French Government for the elementary schools in France, and are more especially used in Paris, where the *Frères de la doctrine Chrétienne* have the charge of the primary schools. We have here also a series of plaster models, one or two of which are on the table. The diagrams are divided into two parts: one of them treats of pure geometry, and contains examples of lines perpendicular to and forming angles with each other, polygons, circles, ellipses, and so forth. Next come a few more difficult examples, such as prisms, pyramids, and a few little examples of machines, such as nuts, thumb-screws, &c. Then, lastly, come a few instances of architectural drawing. Examples of all of these are given, so as to enable the beginner to understand at once the use of plan, elevation, and section. All that I have referred to is supposed to be taught in one year. In the second year comes this second portion, of which there are but a few examples here. In that year the student is taught—first, shading; and here are some examples, showing the theoretical drawing of the shadows of various bodies. Then a selection of stone-cutting, which is a more important subject, and to one example I would ask your attention, because we have before us a model made from it. The students, as they

draw these, make the models themselves. For instance, one figure is what is called a Marseilles arch, and here is a model in exactly the same form, cut out in plaster; in fact, it is executed by the students from the drawing. There are several other examples of the same kind. Here, again, is a drawing in perspective of a groined arch, and a model made of it, by the students, showing how admirably they are taught. There are, besides, a variety of other examples, so that by the end of the second year the student becomes perfectly familiar with all this class of drawing.

We should remember, with regard to perspective, that in these countries it is, no doubt, taught in its truest manner, but it is rather complicated—it is given its fullest development—whereas in these schools in France, there is a very simple system of perspective taught, based upon Descriptive Geometry, and known as that of Monge, one of the founders of the Polytechnic School in France. Although it is hardly necessary to explain this method to you, still, as it is an excellent way of teaching beginners, I will say one word upon it. You draw a ground line, X Y, and a line perpendicular to it, supposed to represent the picture plane. Now, let an object, say a cube, be standing on the ground, and assume S, S′ as the position of the observer's eye. By drawing lines from S and S′ to the plan and elevation of the cube, and determining their intersections with the picture plane, we obtain, rotating this last in plan round *o*, the true perspective of the cube, the sight point being opposite the point S″. The vanishing points are similarly determined, if necessary.

Now, to return to the collection of models before us. The Loan Exhibition contains no models whatever of pure descriptive geometry; and as it is very often difficult to render the position of lines and planes in space apparent to the minds of beginners, I have found a small collection, which I bought in Paris some years ago, very convenient for this purpose. It is of a very simple kind, made by Rouvet, of the Quai de l'Horloge. There is a book pretty well known by those who have studied this subject, called *Lefebure de Fourcy's Elementary Treatise on Descriptive Geometry*, which is one of the best of its kind; and this collection of models is made to

demonstrate the early problems of that work. These boards represent the horizontal and vertical planes, and the strings represent the lines in space. Then there are lines drawn on the boards themselves, which are the projections of these lines; so that with a very little trouble the student can determine at once the meaning of the traces of planes, the traces and projections of lines. There is here also a very convenient little apparatus intended to explain plan and elevation, and also a means of obtaining a second elevation. All these are obtained at small cost, and certainly serve as a good commencement to this study. I should mention that De Fourcy's work has been partly translated in Weale's series, and also by Hall; I think it is the very best *elementary* work on descriptive geometry.

There is another point with respect to descriptive geometry to which I must advert, and that is that the student of analytical geometry is greatly helped by a knowledge of it. For instance, if you are given the equation of lines or planes in space, you can always draw them by laying down co-ordinate planes and a ground line.

But as time presses I must hurry on to this beautiful series of models made by M. Fabre de Lagrange, from M. Ollivier's lectures, which belong to the South Kensington Museum. These models, although numerous individually, are not very numerous in class. They are all what are called ruled surfaces. I should perhaps recall to your minds what the ruled surface is. It is the term applied to every surface, whether twisted or developable, which is described by the movement of a right line in space, so that at every point in such a surface a ruler may be so placed as to be on the surface itself in all its length. In general, there are two systems of construction; so that not only can you lay the ruler on the surface in one direction, but in two. These models contain a little more than the ordinary ruled surfaces, because there are some intersections as well. I will take a few of the examples. The first one is what is generally termed a twisted plane. It is the hyperbolic paraboloid, and is engendered by a line sliding along two other lines, not in the same plane, and remaining constantly parallel to a given plane. These

models are beautifully arranged. You can shift them so as to change the direction of the generating lines. During the movement in this particular model, the length of the generating lines never varies. I say this particular model, because we shall see that is not the case with the others.

Here is a very pretty model of a surface. If you come to examine it, you will find it similar to the last. You have here a directing line in brass, and here another, and these silk generating lines are kept constantly parallel to a directing plane, while remaining in contact with the directing lines, so that the surface is the twisted plane. Below you will see a little net-work, showing the difference between such a surface as this and its horizontal projection; and you will see also a set of little brass rings giving peculiar intersections which give the name to the surface. Of this class there are several.

Hitherto the lengths of the threads representing generating lines do not vary, but in some of them the lengths vary, and there is a beautiful arrangement made for the purpose. This is a model intended to show the curve formed by the intersection of two cones. In this, and most of the models, when the figure is altered by shifting the brasses, the lengths of the generating lines alter as well. And in order to effect this, there are boxes below which contain little weights: these are attached one to each of the threads which represent generating lines; and these fall lower or rise higher in the boxes according as the curvature alters.

There is another very interesting class of surfaces described here—namely, the conoids. This form of conoid now before you is used for arches. The line which describes the surface, while always in contact with the directing curve and line, remains horizontal.

Here is a very pretty instance of the change of a conoid into another figure. As at present placed, all the generating lines remain in contact with this straight line, and emerge again here, producing a similar conoid in an opposite direction; but letting go this string, and turning the brass ring round, the figure becomes a cylinder. If it were turned the other way it would become a cone.

To render this series complete and clear, these models

require casts to indicate what they are intended to be, and I will show you a series of these casts, not made from these models, but which answer the same purpose.

This model represents the intersection of a cylinder by a plane. The yellow lines represent the plane. The simplest way in which a plane is engendered is by a line moving between two parallel lines. The cylinder is produced by a line moving round a curve and remaining parallel to its initial position. You may take any section of the cylinder you please by shifting this long bar. When you come to have the plane perpendicular to all the generating lines of the cylinder, you have what is called the right section of the cylinder, which in this case would be an ellipse.

I have here a very beautiful illustration of the intersection of two cylinders. You may have already seen it in another form. It is nothing more than a groined arch, and the curve of intersection between these two is the groin of the arch. In this next example of two cylinders, the groin would be what is called a waving groin. These intersections occur often in architecture. This figure, again, can be modified by changing the position of the two ends. These little rings show the form of intersection of the two surfaces.

The next model is composed of a series of lines, which are constantly in contact with two curves, one above and one below. A more ordinary and more useful form of this surface is when the two curves are identical or similar. You will see, as I turn this button, how the figure changes.

If I turn the upper curve round thus, so that these generating lines tend to meet at an imaginary point above the model, you have a cone; as I turn it round further we come to the hyperboloid of revolution; and lastly, as I turn still further, all the points come together at this point, and we have again a cone.

This next model is that of the hyperboloid of revolution of one sheet. It is a very useful figure, for it is one employed for making the teeth of skew-bevel wheels.

Here is another example of the hyperboloid. It is composed of two series of coloured lines—one red, and another green, inclined equally and in opposite directions,

giving an illustration of the two methods of generation of the surface.

The next figure will at first appear strange; but, if you examine it, you will see it is exactly similar to the first one I showed you. It is a hyperbolic paraboloid tangent to a twisted surface. At the same time you will see here a plane tangent to the same surface, showing the relation between a plane tangent and a hyperbolic paraboloid tangent. I should remind you that plane tangents are not necessarily tangents to the whole surface. You will see that the plane tangent cuts right through the surface; but the hyperbolic paraboloid is a complete tangent to the surface considered.

These figures are called of one sheet or of two sheets, according as they are of one or two parts. This is called a hyperboloid of one sheet. Supposing you make the hyperbola which I here describe on the board rotate round its vertical axis thus, you will have the hyperboloid of one sheet; if, on the contrary, you make it rotate round the horizontal axis, you will have two complete surfaces of revolution, or a hyperboloid of two sheets.

Besides these figures, there is another very pretty model of intersection I have to draw your attention to. Beginners are very apt to confuse the various intersections of cylinders and cones; and here are two very beautiful examples of the intersection of two cylinders; one where there is but one intersection, and one where one cylinder passes right through another. There are then two intersections. Again these other models are very interesting, showing the change from the plane to the hyperbolic paraboloid. A brief way of describing the paraboloid of one sheet is, that it is a figure described by the sliding contact of a line upon three others, no two of which are in the same plane, and with which it remains constantly in contact.

These next figures are cylinders with cones inside them, and by a simple rotation you change the cone into a cylinder, and the cylinder into the hyperboloid of one sheet.

In conclusion, Prof. Osborne Reynolds exhibits some excellent folding models, showing the traces and the intersections of planes by different coloured lines. Without some such models as these before you, it would be almost impossible

to teach this subject clearly. I have always found it is the elementary portion of descriptive geometry which really is hard for beginners to understand; but once that is passed there is little difficulty. The real object of learning descriptive geometry is to study designing; and I thought it as well, in putting together all these models, to add besides a few examples of German and other models of surfaces, illustrating those last mentioned, in plaster of Paris. All these are simple intersections of cylinders, prisms, cones, and some of these peculiar surfaces with their generating lines upon them. As you see them there, so they occur in space; but once the student has learned these in his elementary course, he should think no more of them, except to be able to apply them when required. Unfortunately, in my experience of a good many years spent in teaching these subjects, I have found students are extremely backward in understanding descriptive geometry, and I should impress upon you that it is absolutely essential, not only to know some plane geometry, but to know also a good deal of solid, and some analytical geometry.

THE LAWS OF FLUID RESISTANCE.

BY W. FROUDE, ESQ., LL.D., F.R.S.

I PROPOSE to consider those principles of fluid motion which influence what is termed the "resistance" of ships. By the term resistance, I mean the opposing force which a ship experiences in its progress through the water. Considering how great an expenditure, whether of sail or steam power, is involved in overcoming this resistance, it is clearly most important that its causes should be correctly appreciated.

This subject is a branch of the general question of the forces which act on a body moving through a fluid, and has within a comparatively recent period been placed in an entirely new light by what is commonly called the theory of stream-lines.

This theory as a whole involves mathematics of the highest order, reaching alike beyond my ken and my purpose; but so far as we shall have to employ it here, in considering the question of the resistance of ships, its principles are perfectly simple and are easily understood without the help of technical mathematics; and I will endeavour to explain the course which I have myself found most conducive to its apprehension.

In order, however, to show you clearly what light the theory of stream-lines has thrown on the question, I must first describe the old method of treating it, which is certainly at first sight the most natural one, and we shall thus see what germs of truth that method contained, and how far these were developed into false conclusions.

It is a crude but instinctive idea, that the resistance experienced either by a ship or by a submarine body, such as a fish, moving through water, is due to the necessity of the body ploughing or forcing or cleaving a passage for itself through the water; that it has to drive the water out of its way and then to draw it in again after itself.

When, however, an attempt was made to deal with the matter in a scientific manner, it was seen that an explanation was needed of how it was that water required force to move it out of the way. For it may naturally be asked, How can there be reaction or resistance in a perfectly mobile material such as water seems to be? We can understand earth, for instance, resisting a ploughshare dragged through it, and we can understand that even a perfectly thin flat plane would make resistance if dragged edgeways through a sea of sand, or even through a sea of liquid mud, owing to the friction against its sides. But water appears, at first sight, altogether unlike this, and seems totally indifferent to change of form of any kind. If we stir water, the different currents seem to flow freely past one another, as if they would go on flowing almost for ever without stopping. But we find, that although to push a thin oar blade through the water edgeways seems to require no force, yet, if we push it flatways, as in rowing, it offers a considerable reaction. The distinction, then, which suggests itself is that the particles of water, although they offer no resistance to anything merely sliding past them, offer great resistance to anything pushing against them, because the thing which is pushing against them sets them in motion out of its way, and to set anything heavy in motion requires the exertion of force to overcome what is called its inertia.

This, then, appears *prima facie* to be the characteristic of water, that to set the particles in motion, or what is the same thing, to divert them from a straight path, requires force to overcome their inertia, although, when once set in motion, they are able to glide freely past one another, or past a smooth surface. This supposition embodies the natural conception of a fluid, and if it were absolutely exemplified in water, then water would be what we should call a perfectly frictionless fluid.

Now, though water is not absolutely frictionless, yet it

is true that in many of the more familiar ways of handling it, the forces developed by its slight frictional qualities are small compared to those due to its inertia, and it is therefore not surprising that those who theorised on the resistance of ships thought it quite accurate enough to treat of the effect of the inertia only, and to neglect the comparatively small frictional qualities.

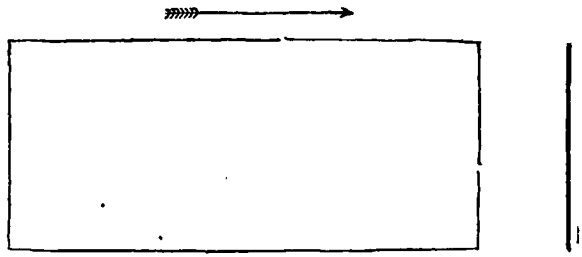


FIG. 1.—ELEVATION. CROSS SECTION.

It was assumed, then, for the purposes of calculation, that the fluid, being frictionless, would offer no resistance to a perfectly thin, flat, smooth plane, such as that shown in Fig. 1, moving edgeways through it, since this would in no way tend to set its particles in motion. But it is obvious that a ship, or fish, or other body, such as that shown in Fig. 2, moving through the water, has to be continually setting the particles of water in motion, in order, first to

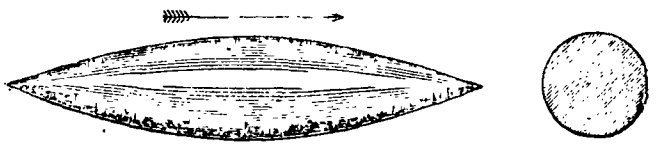


FIG. 2.—ELEVATION. CROSS SECTION.

get them out of its way, and afterwards to close them together again behind it, and that the inertia of the particles thus set in motion will supply forces reacting against the surface of the body. And it seemed certain, at first sight, that these reactions or forces on the surface of the body would necessarily so arrange themselves as to constitute resistance.

On this view, various formulæ were constructed by mathematicians to estimate these reactions, and to count up the sum-total of resistance which they would cause to a ship or moving body of any given form. These formulæ were not all alike, but they were mostly based on the supposition that the entire forward part of the body had to exert pressure to give the particles motion outwards, and that the entire afterpart had to exert suction to give them motion inwards, and that there was, in fact, what is termed *plus* pressure throughout the head-end of the body, and *minus* pressure or partial vacuum throughout the tail-end. And as it seemed that the number of particles which would have to be thus dealt with would depend on the area of maximum cross section of the body, or area of ship's way, as it was sometimes termed, the resistance was supposed to bear an essential proportion to the midship section of the ship. This idea has sometimes been emphatically embodied in the proposition that the work a ship has to do in performing a given voyage is to excavate in the surface of the sea, from port to port, a canal, the cross section of which is the same as the midship section of the ship.

This theory of resistance was at first sight natural and reasonable; it was generally admitted for many years to be the only practicable theory, and was embodied in all the most approved text-books on hydraulics and naval architecture. But when the theory of stream-lines was brought to bear upon the question, then it was discovered that the reactions, which the inertia of the fluid would cause against the surface of the body moving through it, and which were supposed to constitute the resistance, arranged themselves in a totally different manner from what had previously been supposed, and that, therefore, the old way of estimating their total effect upon the ship was fundamentally wrong. How wrong, I can best tell you by stating that according to the theory of stream-lines a submerged body, such as a fish, for example, moving at a steady speed through the assumed frictionless fluid, would experience no resistance at all. In fact, when once put in motion it would go on for ever without stopping.

The revelation, then, which was brought about by the application of the stream-line theory to the question, amounted to this, that the approved formulæ for esti-

mating the resistance of bodies moving through water were not only wrong in detail, but that the supposed cause of resistance, with which alone they professed to be dealing, was in reality no cause at all; and that the real cause of resistance, whatever it might be, was entirely left out.

It is easy to imagine how fruitful, in false aims and false principles of nautical construction, would be the assignment of the resistance of ships to a supposed cause which has no existence at all. And the old theory, though now discarded by scientific men, has obtained such a hold on the minds of the general public, that I hope you will excuse my devoting considerable space to its refutation.

I will now briefly sketch an elementary view of the stream-line theory so far as it is relevant to our present purpose. Let it be understood that I am still dealing only with the supposed frictionless fluid; that for reasons which will hereafter appear, I am dealing not with a ship at the surface, but with a submerged body; and that I am supposing it to be travelling at a steady speed in a straight line. I am going to prove to you that under these circumstances the inertia of the fluid which has to be set in motion to make way for the body, will cause no resistance to it. Not that such inertia will cause no pressures and suctions acting upon the surface of the body; far from it; but that the pressures and suctions so caused must necessarily so arrange themselves, that the backward forces caused to the body on some parts of its surface will be neutralised by the forward forces caused on other parts. In effect, although the inertia of the fluid resists certain portions of the body, it propels the other portions of the body with a precisely equal force.

In showing how this comes about, I prefer to substitute for the submerged body moving through a stationary ocean of fluid, the plainly equivalent conception of a stationary submerged body surrounded by a moving ocean of fluid. The proposition that such a body will experience no total endways push from the fluid flowing past it arises from a general principle of fluid motion, which I shall presently put before you in detail, namely, that to cause a frictionless fluid to change its condition of flow in any manner whatever, and ultimately to return to its original condition of

flow, does not require, nay, does not admit of, the expenditure of any power; whether the fluid be caused to flow in a curved path, as it must do in order to get round a stationary body which stands in its way; or to flow with altered speed, as it must do in order to get through the local contraction of channel which the presence of the stationary body practically creates. Power, it may indeed be said, is being expended, and force exerted to communicate certain motions to the fluid; but that same power is also being given back, and the force counterbalanced, where the fluid is yielding up the motion which has been communicated to it, and is returning to its original condition.

In commencement, I will illustrate these two actions by considering the behaviour of fluid flowing through variously-shaped pipes; and I will begin with a very simple instance, which I will treat in some detail, and

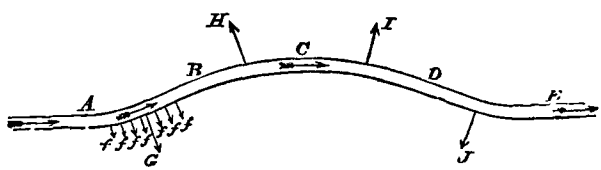


FIG. 3.

which will serve to show the nature of the argument I am about to submit to you.

Suppose a rigid pipe of uniform sectional area, of the form shown in Fig. 3, something like the form of the water-line of a vessel.

The portions A B, B C, C D, D E are supposed to be equal in length, and of the same curvature, the pipe terminating at E in exactly the same straight line in which it commenced at A, so that its figure is perfectly symmetric on either side of C, the middle point of its length.

Let us now assume that the pipe has a stream of frictionless fluid running through it from A towards E, and that the pipe is free to move bodily endways.

It is not unnatural to assume at first sight that the tendency of the fluid would be to push the pipe forward, in virtue of the opposing surfaces offered by the bends in it—

that both the divergence between A and C from the original line at A, and the return between C and E to that line at E, would place parts of the interior surface of the pipe in some manner in opposition to the stream or flow, and that the flow thus obstructed would drive the pipe forward; if however we endeavour to build up these supposed causes in detail, we shall find the reasoning to be illusory, and I will now trace the results which can be established by correct reasoning.

The surface being assumed to be smooth, the fluid, being a frictionless fluid, can exercise no drag by friction on the side of the pipe in the direction of its length, and in fact can exercise no force on the side of the pipe, except at right angles to it. Now the fluid flowing round the curve from A to B will, no doubt, have to be deflected from its course, and its inertia, by what is commonly known as centrifugal action, will cause pressure against the outer side of the curve, and this with a determinable force. The magnitude and direction of this force at each portion of the curve of the pipe between A and B are represented by the small arrows marked *f*; and the aggregate of these forces between A and B is represented by the larger arrow marked G. In the same way the forces acting on the parts B C, C D, and D E are indicated by the arrows H, I, and J; and as the conditions under which the fluid passes along each of the successive parts of the pipe are precisely alike, it follows that the four forces are exactly equal, and, as shown by the arrows in the diagram, they exactly neutralise one another in virtue of their respective directions; and therefore the whole pipe from A to E, considered as a rigid single structure, is subject to no disturbing force by reason of the fluid running through it.

Though this conclusion, that the pipe is not pushed endways, may appear on reflection so obvious as to have scarcely needed proof, I hope that it has not seemed needless, even though tedious, to follow somewhat in detail the forces that act, and which, under the assumed conditions, are the only forces that act, on a symmetrical pipe such as I have supposed.

Having shown that in the instance of this special symmetrically curved pipe, the flow of a frictionless fluid through it does not tend to push it endways, I will now

proceed to show that this is also the case whatever may be the outline of the pipe, provided that its beginning and end are in the same straight line.

Assume a pipe bent into a complete circular ring with its end joined, and the fluid within it running with velocity round the circle. The inertia of this fluid, by centrifugal force, exercises a uniform outward pressure on every part of the uniform curve; and this is the only force the fluid can exert. This outward pressure tends to enlarge or stretch the ring, and thus causes a uniform circumferential tension on each part of the ring.

Now take a ring of twice the diameter and suppose the fluid to be running round it with the same linear velocity as before. The diameter of the curve being doubled, and the speed being the same, the outward pressure due to centrifugal force on each linear inch of the ring will be halved; but since the diameter is doubled, the number of linear inches in the circumference of the ring will be doubled. Since, then, we have twice the number of inches acting, each with half the force, the total force tending to enlarge the ring will be unaltered, and the circumferential tension on the ring caused by the centrifugal force of the fluid will be just the same as before.

In the same way we can prove that in any number of rings of any diameters, if the linear velocity of the fluid in each is the same, the circumferential tension caused by the centrifugal force of the fluid will also be the same in each.

Now let us take each of these rings and cut out a piece, and then join all these pieces together so as to form a continuous pipe, as in Fig. 4, and suppose the stream of fluid flowing through the combined pipe, with the same linear velocity as that with which it was before flowing round each of the rings. The fluid in each of the segments will now be in precisely the same condition as when the segment formed part of a complete ring, and will subject each piece of ring to the same strains as before, namely, to a longitudinal tension or strain, and to that only. And since we have already seen that the tension is the same in amount in each ring, the tension will be the same at every point in the combined pipe.

This being so, if we imagine the pipe to be flexible (but

not elastic), and to be fastened at the ends, the pipe, although flexible, will not tend to be disturbed in its shape by the inertia of the fluid which is running through it; because the fluid does not cause any lateral force, but only a longitudinal stretching force, and that the same in

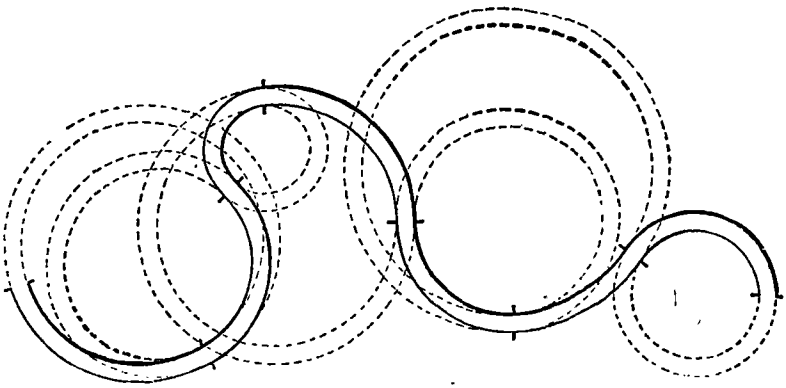


FIG. 4.

amount at every point. And this will clearly be so in a pipe of any outline, because any curve may be made up by thus piecing together short bits of circular arcs of appropriate radii.

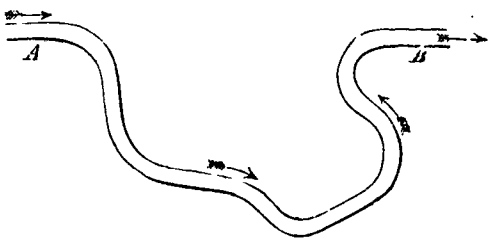


FIG. 5.

Let us then take a flexible pipe having the two ends in the same straight line, but pointing away from one another, as in Fig. 5, the intermediate part being of any outline you please. If the ends are fixed we have seen that the flow of

fluid will not tend to disturb the pipe, and therefore all that will be necessary to hold it in its position will be an equal and opposite tension supplied by the anchorages at the ends, to prevent the ends being forced towards one another. And if, instead of anchoring the ends, we put a strut between them to keep them apart, the pipe thus fitted will require no external force to keep it in position. In other words, whatever be the outline of a pipe, provided its beginning and end are in the same straight line, a frictionless fluid flowing through it will have no tendency to push it bodily endways.

So far I have dealt only with pipes having uniform sectional area throughout their length, an assumption which has been necessary to the treatment pursued, as the velocity has in each case been assumed to be uniform throughout the length of the pipe. I will now proceed to consider the behaviour of fluid flowing through pipes of varying sectional area, and consequently flowing with varying velocity.

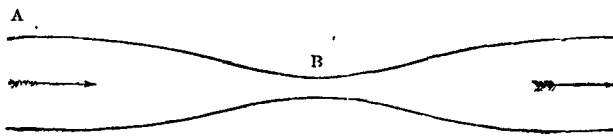


FIG. 6.

It is, I think, a very common impression, that a fluid in a pipe, meeting a contraction of diameter (see left hand of Fig. 6), exercises an excess of pressure against the entire converging surface which it meets, and that conversely, as it enters an enlargement (see right hand of Fig. 6), a relief of pressure is experienced by the entire diverging surface of the pipe. Further, it is commonly thought that there is in the narrow neck of a contracted passage (see Fig. 6) an excess of pressure due to the squeezing together of the fluid at that point.

These impressions are in every respect erroneous; the pressure at the smallest part of the pipe is, in fact, less than that at any other point, and *vice versâ*.

If a fluid be flowing along a pipe A B which has a contraction in it, the forward velocity of the fluid at B must be

greater than that at A, in the proportion in which the sectional area of the pipe at B is less than that at A; and, therefore, while passing from A to B the forward velocity of the fluid is being increased. This increase of velocity implies the existence of a force acting in the direction of the motion, to overcome the inertia of the fluid; that is to say, each particle which is receiving an increase of forward velocity must have a greater fluid pressure behind it than in front of it; for no other condition will cause that increase of forward velocity. Hence a particle of fluid, at each stage of its progress along the tapering contraction, is passing from a region of higher pressure to a region of lower pressure, so that there must be a greater pressure in the larger part of the pipe than in the smaller, the diminution of pressure at each point corresponding with the diminution of sectional area, corresponding, that is to say, with the additional forward velocity assumed by the fluid at each point of its advance along the contraction. Consequently, differences of pressure at different points in the pipe depend solely upon the velocities, or, in other words, on the relative sectional areas of the pipe, at those points.

It is easy to apply the same line of reasoning to the converse case of an enlargement. Here the velocity of the particles is being reduced through precisely the same series of changes, but in an opposite order. The fluid in the larger part of the pipe moves more slowly than that in the smaller, so that, as it advances along the enlargement, its forward velocity is being checked; and this check implies the existence of a force acting in a direction opposite to the motion of the fluid, so that each particle which is being thus retarded must have a greater fluid-pressure in front of it than behind it; thus a particle of fluid at each stage of its progress along a tapering enlargement of a pipe, is passing from a region of lower pressure to a region of higher pressure, the change of pressure corresponding to the change of velocity required. Hence we see that a given change of sectional area will require the same change of pressure, whether the pipe be an enlargement or a contraction.

Therefore, in a pipe in which there is a contraction and a subsequent enlargement to the same diameter as before

(see Fig. 6), since the differences of pressure at different points depend on the differences of sectional area at those points, by a law which is exactly the same in an enlarging as in a contracting pipe, the points which have the same sectional areas will have the same pressures, the pressures at the larger areas being larger, and those at the smaller areas smaller.

FIG. 7.

Precisely the same result will follow in the case of an enlargement followed by a contraction (see Fig. 7).

Were water a frictionless fluid these propositions could be exactly verified by experiment as follows.

Figs. 8 and 9 show certain pipes, the one a contraction followed by an enlargement, the other an enlargement followed by a contraction. At certain points in each pipe

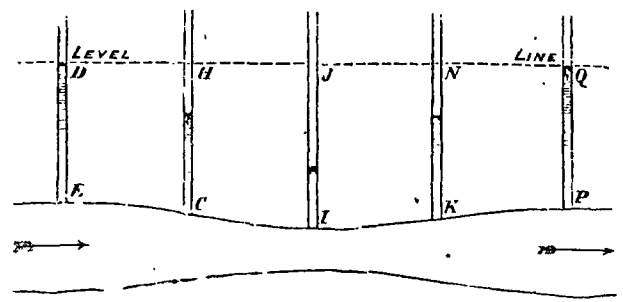


FIG. 8.

there are small holes, communicating with vertical gauge-glasses. The height at which the fluid stands in each of these vertical glasses of course indicates the pressure in the pipe at the point of attachment.

In Fig. 8 the sectional areas at E and P are equal to one another. Those at C and K are likwise equal to one

another, but are smaller than those at E and P. The area at I is the smallest of all. Now, the fluid being frictionless, the pressures at E and P indicated by the heights E D and P Q would be equal, these being greater than C H and K N. C H and K N would also be equal to one another, and would be themselves greater than I J.

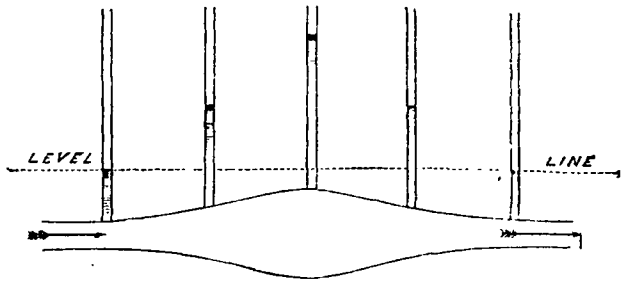


FIG. 9.

The results shown in Fig. 9 are similar in kind, equal pressures corresponding to equal sectional areas.

But if the experiment were tried with water, some of the pressure at each successive point would be lost in friction,

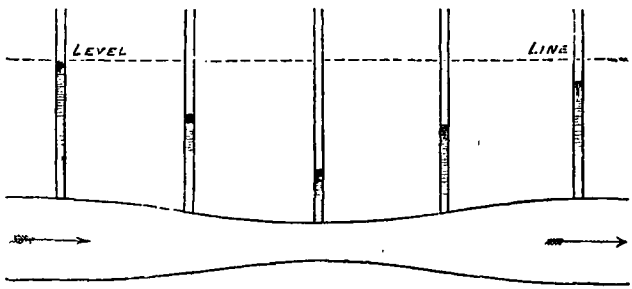


FIG. 10.

and this growing defect in pressure, or "gradient," would be indicated in the successive gauge-glasses in the manner shown in Figs. 10 and 11.

I have here arranged an experiment which conveniently

illustrates these propositions, making allowance for the frictional gradient.

h k l e f g a b c (see Fig. 12) is a continuous series of glass tubes, through which water is flowing from the cistern *n* to the outlet *m*. The cistern is kept full to a certain level. The tube from *h* to *l* is what I have called an enlargement followed by a contraction (like Fig. 7); from *e* to *g*, the diameter is the same throughout; and from *a* to *b*, the tube is a contraction followed by an enlargement (like Fig. 6). Just as in Figs. 8, 9, 10, 11, gauge-glasses are here fitted to the various tubes to show the pressures of the water in them at various points.

Let us first consider the parallel pipe *e g*. If the fluid were frictionless, the diameter being uniform, the pressure

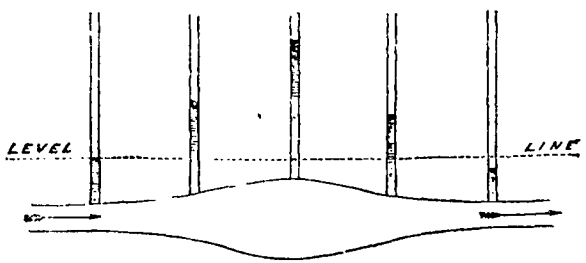


FIG. 11.

would be uniform throughout, and the fluid would stand at the same level in each of the three gauge-glasses. But, owing to the friction, the water surfaces in the three glasses do not come up to a level line, but form a descending line, namely the frictional gradient.

Now take the pipe *a c*. Here the smallest pressure, denoted by the water level at *b'*, is in the middle at *b*, where the diameter is smallest, and the greatest pressure denoted by the water levels at *a'*, *c'*, is at the two ends *a*, *c*, where the diameter is greatest. And if the fluid were frictionless, the pressure at the two ends, which have the same diameter, would be the same, but with water there is, as in the parallel pipe *e g*, a gradient or loss of pressure due to the friction.

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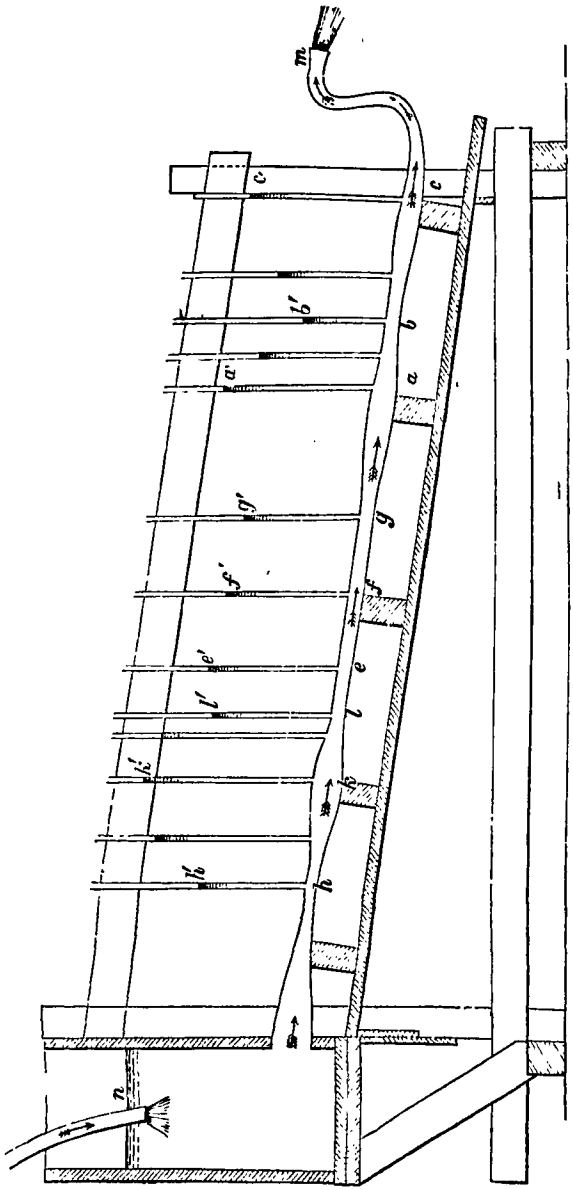


FIG. 12.

The frictional gradient, according to well-known hydraulic rules, has a definite law of variation in terms of diameter and velocity, consequently it has been possible by calculation to so arrange the diameters of the pipes that the parallel pipe *e g* should, according to the rule, have the same frictional gradient as the pipe *a c*, and as we see that the gradients are in fact the same, the result not merely illustrates but verifies the propositions.

In the pipe *h k l* we have the smallest diameter at the two ends *h* and *l*, and the largest diameter at the middle point *k*, and consequently we have the smallest pressures denoted by the water levels at *h'* and *l'*, at the two ends, and the greatest pressure in the middle denoted by the water level at *k'*, and we again have the fall or gradient from end to end due to friction.

These experiments afford a good verification of the proposition which I have just now explained, namely, that in a frictionless fluid flowing through a pipe of varying diameter, the pressure at each point depends on the sectional area at that point, there being equal pressures at the points of equal sectional area. Hence if in the pipe shown in Fig. 13 the areas at all the points marked A are equal, if also the areas at all the points marked B are equal, and so also with those of C and D, then the pressures at all the points A will be the same, the pressures at all the points B will be the same, and so with those at C and D.

Since, then, the pressure at each point depends on the sectional area at that point and on that only, it is easy to show that the variations in pressure due to the flow are not such as can cause any total endways force on the pipe, provided its sectional area at each end is the same.

Take for instance the pipe shown in Fig. 14. The conical portion of pipe A B presents the same area of surface effective for endways pressure as does the conical portion H I, only in opposite directions. They are both subject to the same pressure, being that appropriate to their effective mean diameter J. Consequently the endways pressures on these portions are equal and opposite, and neutralise one another. Precisely in the same way it may be seen that the endways pressures on B C, C D, D E, exactly counteract those on G H, F G, E F; and it may be

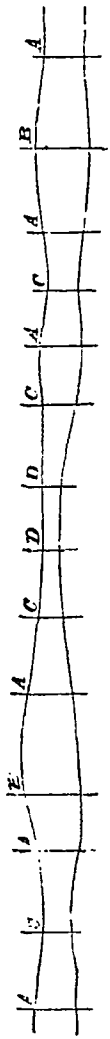


FIG. 13.

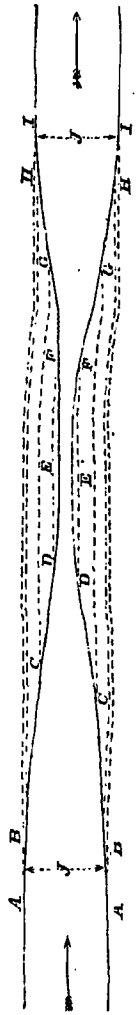


FIG. 14.

similarly shown, that in any combination whatever of enlargements and contractions, provided the sectional area and direction of the pipe at the two ends are the same, the

total endways force impressed on the pipe by the fluid flowing through it must be *nil.*

We see then that a frictionless fluid flowing through a pipe of any form, whether tortuous or of varying diameter, will not tend to push it endways, as long as the two ends of the pipe are in the same straight line, and have the same sectional area; in a word, as long as the speed and direction of flow of the fluid are the same in leaving the pipe as in entering it; and in this compound proposition concerning the flow of fluid through pipes, I have laid the necessary foundation for the treatment of the case of the flow of an ocean of frictionless fluid past a submerged body.

I have dealt with the instance of a single stream of uniform sectional area (and therefore of uniform velocity of flow) inclosed in a pipe of any outline whatever, and I have dealt with the instance of a single stream of varying sectional area and velocity of flow; and in both these cases I have shown that, provided the streams or pipe-contents finally return to their original direction and velocity of flow, they administer no total endways force to the pipe or channel which causes their deviations.

I am now going to deal with a combination of such streams, each to some extent curved and to some extent varying in sectional area, which, when taken together, constitute an ocean of fluid, flowing steadily past a stationary submerged body, see Fig. 15; and here also, since the combination of curved streams surrounding the body, which together constitute the ocean flowing past it, return finally to their original direction and velocity, they cannot administer to the body any endways force.

Every particle of the fluid composing this ocean, as it passes the body, must undoubtedly follow some path or other, though we may not be able to find out what path; and every particle so passing is preceded and followed by a continuous stream of particles all following the same path, whatever that may be. We may then, in imagination, divide the ocean into streams of any size and of any cross section we please, provided they fit into one another so as to occupy the whole space, and provided the boundaries which separate the streams exactly follow the natural courses of the particles.

If we trace the streams to a sufficient distance ahead of the body, we shall there find the ocean flowing steadily on, completely undisturbed by, and, so to speak, ignorant of the existence of the body which it will ultimately have to pass. There, all the streams must have the same direction, the same velocity of flow, and the same pressure. Again, if we pursue their course backwards to a sufficient distance behind the body, we shall find them all again flowing in their original direction; they will also have all resumed their original velocity; for otherwise, since the velocity of the ocean as a whole cannot have changed, we should have a number of straight and parallel streams having different velocities side by side with one another. This, in a frictionless fluid, would be clearly an impossible state of things, for we have seen that in a frictionless fluid the velocities exactly correspond with the pressures, so that if the velocities of these streams were different the pressures would be different, and if the pressures were different the fluid would begin to flow from the greater pressures towards the less, and the streams would thus become curved instead of straight.

Thus, although in order to get past the body these streams follow some courses or other, various both in direction and velocity, settling themselves into these courses in virtue of the various reactions which they exert upon one another and upon the surface of the body, yet ultimately, and through the reverse operation of corresponding forces, they settle themselves into their original direction and original velocity. Now the sole cause of the original departure of each and all of these streams from, and of their ultimate return to, their original direction and velocity, is the submerged stationary body; consequently the body must receive the sum total of the forces necessary to thus affect the streams. Conversely this sum total of force is the only force which the passage of the fluid is capable of administering to the body. But we know that to cause a single stream, and therefore also to cause any combination or system of streams, to follow any courses changing at various points both in direction and velocity, requires the application of forces the sum total of which in a longitudinal direction is *nil*, provided that the end of each stream has the same direction and velocity as

the beginning. Therefore the sum total of the forces (in other words the only force) brought to bear upon the body by the motion of the fluid in the direction of its flow, is *nil.*

Another instructive way of regarding the same problem is this. Suppose each and every one of the streams into which we have subdivided the ocean to be inclosed in an imaginary rigid pipe made exactly to fit it, throughout, the skin of each pipe having no thickness whatever. The innermost skin of the innermost layer of pipes (I mean that layer which is in contact with the side of the body), the innermost skin, I say, of this layer is practically neither more nor less than the skin or surface of the body. The other parts of the skins of this layer, and all the skins of all the other pipes, simply separate fluid from fluid, which fluid *ex hypothesi* would be flowing exactly as it does flow if the skins of the pipes were not there; so that, in fact, if the skins were perforated, the fluid would nowhere tend to flow through the holes. Under these circumstances the flow of the fluid clearly cannot bring any force to bear on any of the skins of any of the pipes, except on the innermost skin of the innermost layer. Now we know that the fluid flowing through this system of pipes administers no total endways force to any one of the pipes or to the system as a whole. But it produces, as we have just seen, no force whatever upon any of the skins which separate fluid from fluid; consequently if these are removed altogether, the force administered to the remainder of the system, will be the same as is administered to the whole system, namely, no total endways force whatever. But what is this remainder of the system which has no total endways force upon it? Simply the surface of the body, which is formed, as I have already said, by the innermost skins of the innermost layer of pipes. Therefore no total endways force is administered to the body by the flow of the fluid.

I have now shown that an infinite ocean of frictionless fluid flowing past a stationary submerged body cannot administer to it any endways force, whatever be the nature of the consequent deviations of the streams of fluid. The question, what will be in any given case the precise configuration of those deviations, is irrelevant to the proof I

have given of this proposition. Nevertheless it is interesting to know something, at least, of the general character which these deviations, or "stream-lines," assume in simple cases; therefore I show some in Figs. 15 and 16, which are drawn according to the method explained by the late Professor Rankine.

The longitudinal lines represent paths along which particles flow; they may therefore be regarded as boundaries of the streams into which we imagined the ocean to be divided.

We see that, as the streams approach the body, their first act is to broaden, and consequently to lose velocity, and therefore, as we know, to increase in pressure. Presently they begin to narrow, and therefore quicken, and diminish in pressure, until they pass the middle of the body, by which time they have become narrower than in their original undisturbed condition, and consequently have a greater velocity and less pressure than the undisturbed fluid. After passing the middle they broaden again until they become broader than in their original condition, and therefore have less velocity and greater pressure than the undisturbed fluid. Finally, as they recede from the body they narrow again until they ultimately resume their original dimension, velocity, and pressure. Thus, taking the pressure of the surrounding undisturbed fluid as a standard, we have an excess of pressure at both the head and stern ends of the body, and a defect of pressure along the middle.

We proved just now that, taken as a whole, the pressures due to the inertia of the fluid could exert no endways push upon the stationary body. We now see something of the way in which the separate pressures act, and that they do not, as seems at first sight natural to expect, tend all in the direction in which the fluid is flowing; on the contrary, pressure is opposed to pressure, and suction to suction, and the forces neutralise one another and come to nothing; and thus it is that an ocean of frictionless fluid, flowing at steady speed past a stationary submerged body, does not tend to push it in the direction of the flow. This being so, a submerged body travelling at a steady speed through a stationary ocean of frictionless fluid will experience no resistance.

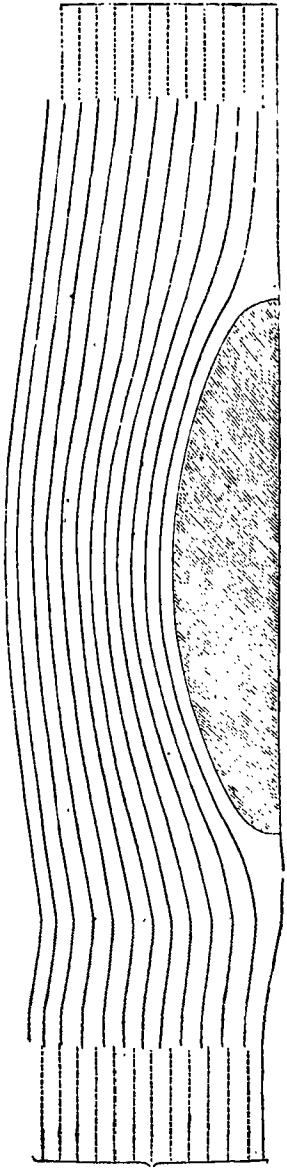


Fig. 15.

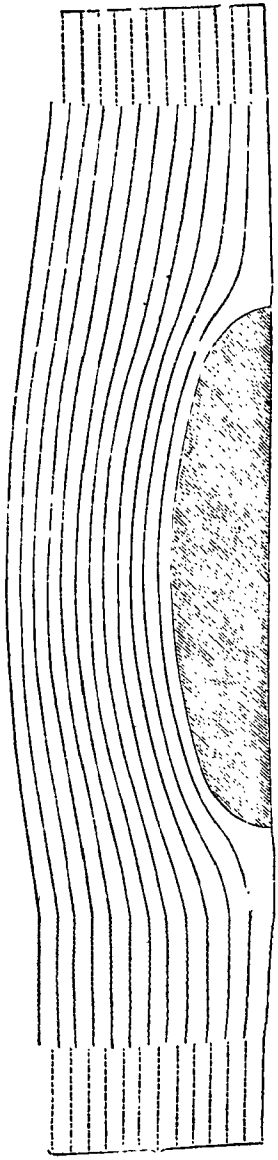


Fig. 16.

Since then a frictionless fluid would offer no resistance to a submerged body moving through it, we have next to consider what are the real causes of the resistance which such a body experiences when moving through water. The difference between the behaviour of water and that of the frictionless fluid is twofold, as follows:

First, the particles of water, unlike those of a frictionless fluid, exert a drag or frictional resistance upon the surface of the body as they glide along it. This action is commonly called surface-friction or skin-friction, and its amount in any given case can be calculated from general experimental data. The resistance due to the surface-friction of a body such as that which we have been considering is practically the same as that of a plane surface of the same length and area, moving at the same speed edgeways through the water.

The second difference between the behaviour of water and that of the imaginary frictionless fluid surrounding the moving submerged body, is that the mutual frictional resistance experienced by the particles of water in moving past one another somewhat hinders the necessary stream-line motions, alters their nice adjustment of pressures and velocities, defeats the balance of forward and backward forces acting against the surface of the body, and thus induces resistance. This action, however, seems imperceptible in forms of fairly easy shape such as that shown in Fig. 2, and only operates tangibly where there are angular features, or very blunt sterns, like the blunt round tail, for instance, of the bodies shown in Figs. 15 and 16. In such a case, the stream-lines, instead of closing in round the stern, as shown in the figures, form a swirl or eddy, from which it results that the excess of pressure which would exist at the tail-end in a frictionless fluid, and which would there counterbalance the similar excess of pressure at the nose of the body, becomes in water greatly reduced, and in part converted into negative pressure, and thus a very great resistance may result. It is worth mentioning, however, that it is blunt tails rather than blunt noses that cause these eddies, and thus a body with one end round and the other sharp, no doubt experiences least resistance when going with the round end first.

I call this source of resistance "eddy-making resistance,'

and, as I have said, it will be imperceptible in forms of fairly easy shape, such, for example, as Fig. 2. Such a form of submerged body will experience practically no resistance except that due to surface-friction, and will therefore experience practically only the same total resistance as a thin plane, like Fig. 1, moving edgeways, which possesses the same area of wetted skin. In fact, we may say generally, that all submerged bodies of fairly fine lines experience no resistance except surface-friction.

I have hitherto, throughout the whole of this reasoning, been dealing with submerged bodies only, by which I mean bodies travelling at a great depth below the surface of the fluid; and I have shown the sole causes of their resistance to be the two I have termed respectively surface-friction and eddy-making resistance. But when we come to the case of a ship, or any other body travelling at or indeed

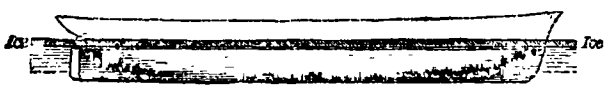


FIG. 17.

near the surface, we find a new cause of resistance introduced; a cause, the consideration of which is often of most vital importance in the design of the forms of ships, and which renders the question of the form of least resistance for a ship, entirely different from that of the form of least resistance for a submerged body. This new cause of resistance, like the eddy-making resistance, operates by altering the stream-line motions and defeating their balance of forward and backward forces. It arises as follows:

Imagine a ship travelling at the surface of the water, and first let us suppose the surface of the water to be covered with a sheet of rigid ice, and the ship cut off level with her water-line, so as to travel beneath the ice, floating, however, exactly in the same position as before (see Fig. 17). As the ship travels along, the stream-line motions will be the same as for a submerged body, of which the ship may be regarded as the lower half; and the ship will move without resistance, except that due to the two causes

I have just spoken of, namely surface-friction and eddy-making resistance. The stream-line motions being the same in character as those we have been considering, we shall still have at each end an excess of pressure, and along the sides a defect of pressure, which will tend the one to force up the sheet of ice and the other to suck it down. If now we remove the ice, the water will obviously rise in level at each end, in order that excess of hydrostatic head may afford the necessary reaction against the excess of pressure, and the water will sink by the sides, in order that defect of hydrostatic head may afford reaction against the defect of pressure.

The hills and valleys which thus commence to be formed in the water are, in a sense, waves, and though originating in the stream-line forces of the body, yet when originated, they come under the dominion of the ordinary laws of wave-motion, and to a large extent behave as independent waves; and in virtue of their independent action they modify the stream-line forces which originated them, and alter the pressures which are acting upon the surface of the ship.

The exact nature of this alteration of pressure, in any given case, we have no means of predicting; but we can be quite sure it must operate to alter the balance of forward and backward forces in such a way as to cause resistance; for we see that the final upshot of all the different actions which take place is this—that the ship in its passage along the surface of the water has to be continually supplying the waste of an attendant system of waves, which, from the nature of their constitution as independent waves, are continually diffusing and transmitting themselves into the surrounding water, or, where they form what is called broken water, crumbling away into froth. Now, waves represent energy, or work done, and therefore all the energy represented by the waves wasted from the system attending the ship is so much work done by the propellers or tow ropes which are urging the ship. So much wave-energy wasted per mile of travel is so much work done per mile, and so much work done per mile is so much resistance.

The surface of the water thus admits of an escape, as it were, of the pressures which arise from the inertia of

the particles of the fluid which have to be set in motion by the body. But so far from thereby rendering less obstruction to the passage of the body, these pressures are enabled by that very escape to result in a resistance, which, if they were confined by the fluid overhead, as with a submerged body, they would have been unable to produce; in fact at the surface the particles are able to escape the duty of restoring to the body the power which the body employed to set them in motion. There can be no doubt that in this way a fish, when swimming so close to the surface as to make waves, experiences more resistance than when deeply immersed.

It is worth remark that this cause of resistance, "wave-genesis" or "wave-making resistance," as it has been termed, would be equally a cause of resistance in a frictionless fluid, and it is for this reason that in proving to you just now that a body would experience no resistance in moving through a frictionless fluid, I limited the case to that of a submerged body. It is true that in a frictionless fluid the wave system generated by a ship would not waste away, as in water, by its internal friction; but it would none the less be diffused into the surrounding fluid, and thus, as the ship proceeded, she would cover a larger and larger area of ocean surface with the waves she was making.

Having arrived at this point, I think it will be useful briefly to review the several cases of motion through fluid, in order to trace where the several causes of resistance we have dealt with come into operation.

Case I.—A plane moving edgeways through frictionless fluid. Here there will be no resistance.

Case II.—A plane moving edgeways through frictional fluid. Here there will be resistance due to surface friction.

Case III.—A submerged body moving through frictionless fluid. The inertia of the fluid undergoing stream-line motion, causes excess of pressure at the two ends, and defect of pressure along the middle. The forward and backward pressures balance one another, and therefore cause no resistance.

Case IV.—A submerged body moving through frictional fluid. Here there is resistance due to surface friction. Also, if the body is abrupt enough to cause eddies, part of

the excess of pressure at the tail-end will be converted by the friction of the particles of fluid into defect of pressure, and so will destroy the balance between the forward and backward pressures, thus causing eddy-making resistance.

Case V.—A body moving through frictionless fluid, but at or near the surface. The direct pressures on the surface of the body are altered by the operation of the wave system which has been created, thus destroying the balance of forward and backward forces, and introducing wave-making resistance.

Case VI.—A body moving through frictional fluid at or near the surface. Here, surface-friction, eddy-making resistance, and wave-making resistance will act in combination, and will together make up the total resistance.

Having this reviewed the several operations which will combine to cause resistance to a ship moving at the surface of the water, it will be interesting to see in what proportion they are combined in an actual ship of ordinary form; and to take a single instance I show the "curves of resistance," as they are called, of the SS. *Merkara*, a mercantile ocean steamship of 3,980 tons. It is perhaps necessary to explain that a curve of resistance is a diagram constructed to show at a glance the resistance at any speed, so that if any point be taken on the scale of speed forming the base-line, the ordinate or vertical height from the point to the curve above, measured by the scale of force, will show the amount of resistance at that speed. Thus, in Fig. 18, where the uppermost line represents the total resistance of the ship, we see that at a speed of twelve knots the resistance as indicated by the height up to the line is 9·3 tons.

The plain line on Fig. 18 is the curve of total resistance of the *Merkara*, deduced from experiments made with a model of that ship. The lowest of the two dotted lines is the curve of surface-friction resistance of the ship, calculated from experiments made upon the resistance of thin planes moving edgeways through water. The space between the foregoing line and the dotted line immediately above it, represents the amount of resistance due to eddy-making, deduced from data which it would take too long to describe here. The space between this upper dotted line and the plain line above it is the wave-making resistance.

We see then, that with this ship the eddy-making resistance is about eight per cent. of the surface-friction, at all speeds. We see further that at eight knots the wave-making resistance is practically *nil*, that at eleven knots it is only twelve per cent. of the whole resistance at that speed, and that at thirteen knots, which is the maximum speed of the ship, it is seventeen per cent. of the whole. As we go further up in the scale of speed the wave-making resistance mounts up very largely, and at nineteen knots is fully sixty per cent. of the whole resistance.

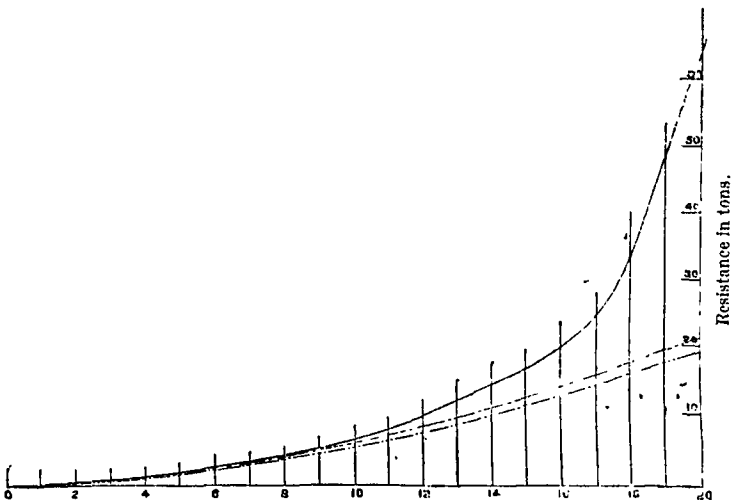


FIG. 18.—Speed in knots per hour.

The curve of resistance here given may be taken as a fair sample of those of ships of good build. It may be said generally that the eddy-making resistance is a comparatively small amount, and that it bears at all speeds nearly a constant proportion to the surface-friction. The wave-making resistance, on the contrary, always increases with increase of speed at a more rapid rate than the surface-friction, being generally *nil* at a very low speed, and becoming, at very high speeds, more than half of the whole resistance. Large ships, however, do not often attain, under steam,

speeds at which the wave-resistance is more than some forty per cent. of the whole.

It is a point worth noticing here, what an exceedingly small force after all is the resistance of a ship, compared with the apparent magnitude of the phenomena involved. Scarcely any one, I imagine, seeing for instance the new frigate *Shah* steaming at full speed, would be inclined at first sight to credit, what is nevertheless the fact, that the whole propulsive force necessary to produce that apparently tremendous effect is only 27 tons, in fact less than one two-hundredth part of the weight of the vessel. And of this small propulsive force, at least 15 tons, or more than one-half, is employed in overcoming surface-friction simply.

Thus, although the vessel carries at her bow a wave seven feet high, the forces which produce this are so far neutralised by other similar forces that the whole of her resistance, exclusive of surface-friction, might be represented by the sternward pressure on her bow which would be due to a single wave fourteen inches high. Indeed, a wave thirty inches high would represent a sternward pressure equal to the whole resistance of the ship.

The truth is, that the forces which are at work, namely the stream-line pressures due to the inertia of the fluid, are indeed very great; what we have to deal with, in the shape of eddy-making or wave-making resistance, is nothing but a minute difference or defective balance between these great forces, and fortunate it is that they balance as well as they do. With a well-shaped ship at moderate speed we have scarcely any resistance but skin friction, for the balance of stream-line pressures is almost perfect; but nevertheless they are all the while in full operation, a forward force counteracting a backward force, each equal to perhaps five times the existing total resistance of the ship. We can easily imagine, then, that when we once begin to tamper with this balance, we may produce unexpectedly great resistance; and thus when we are dealing with speeds at which the wave-making resistance comes into play, a small variation in form may cause a comparatively large variation in the wave-making resistance. It is this fact which gives the wave-making resistance such a vital importance in connection with the designing of ships; but unfortunately, although the surface-friction element of resistance is easily

calculated in all cases from general experimental data, neither theory nor general experiment have as yet supplied means of calculation applicable to the wave-making resistance. In the absence of this knowledge we have to rely on direct experiments with different forms of vessels, and to supply these is one of the objects of the experiments upon the resistances of models of various forms which I am now conducting for the Admiralty.

By these experiments I hope not only to obtain a great many comparisons, showing at once the superiorities of some forms over others; but to deduce general laws by which the influence of variation of form upon wave-making resistance may be predicted. Already, indeed, some most instructive propositions concerning the operations of this cause of resistance have shaped themselves; but it would take far too long to describe them in this discourse. I will merely refer to one broad principle which underlies most of the important peculiarities of the wave-making element of resistance.

We have seen that the waves originate in the local differences of pressure caused in the surrounding water by the vessel passing through it; let us suppose, then, that the features of a particular form are such that these differences of pressure tend to produce a variation in the water-level shaped just like a natural wave, or like portions of a natural wave of a certain length.

Now an ocean wave of a certain length has a certain appropriate speed, at which only it naturally travels, just as a pendulum of a certain length has a certain appropriate period of swing natural to it. And just as a small force recurring at intervals corresponding to the natural period of swing of a pendulum will sustain a very large oscillation, so, when a ship is travelling at the speed naturally appropriate to the waves which its features tend to form, the stream-line forces will sustain a very large wave. The result of this phenomenon is, that as a ship approaches this speed the waves become of exaggerated size, and run away with a proportionately exaggerated amount of power, causing corresponding resistance. This is the cause of that very disproportionate increase of resistance experienced with a small increase of speed when once a certain speed is reached, an instance of which is exhibited at a

speed of about eighteen knots in the curve of resistance shown in Fig. 18.

We thus see that the speed at which the rapid growth of resistance will commence, is a speed somewhat less than that appropriate to the length of the wave which the ship tends to form. Now, the greater the length of a wave is, the higher is the speed appropriate to it; therefore the greater the length of the waves which the ship tends to form, the higher will be the speed at which the wave-making resistance begins to become formidable. We may therefore accept it as an approximate principle, that the longer are the features of a ship which tend to make waves, the longer will be the waves which tend to be made, the higher will be the speed she will be able to go before she begins to experience great wave-making resistance, and the less still will be her wave-making resistance at any given speed.

This principle is the explanation of the extreme importance of having at least a certain length of form in a ship intended to attain a certain speed; for it is necessary, in order to avoid great wave-making resistance, that the "wave features," as we may term them, should be long in comparison with the length of the wave which would naturally travel at the speed intended for the ship.

Time will not admit of my describing to you in detail how the principles I have been explaining affect the practical question of how to shape ships. I must leave you to imagine for yourselves, if you feel interested in following up the question, how the desirability of length of "wave features," for lessening wave-resistance, is to a greater or less extent counteracted by the desirability of shortness of ship for lessening surface-friction; and how in many other ways a certain variation of form, while it is a gain in one way is a loss in another, so that in every case the form of least resistance is a compromise between conflicting methods of improvement.

My principal object has been to combat the old fallacy of "head-resistance," as it has been sometimes called, due to the inertia of the water acting against the area of the ship's way. I hope I have made it clear to you, that the inertia of a frictionless fluid could offer no opposing force to a submerged body of any shape moving through it, for

that the forces there developed by the inertia against the body must of necessity push it forwards exactly as much as they push it backwards, and that when the body is moving through a frictional fluid, or when it is moving at the surface of a fluid, this balance is only more or less destroyed through the operation of conditions which are totally independent of the area of midship section or area of ship's way.

For this reason, the only instances I have time to give you of the application of our knowledge of the causes of resistance to practical questions shall be directly applicable as illustrations of the fallacy of the midship section theory.

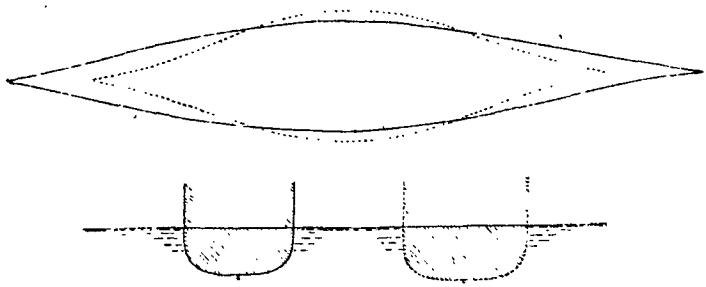


FIG. 19.

Let us suppose that Fig. 19 represents the respective water-lines of two vessels of the same tonnage but of different proportions of length to breadth. Now it is true that the shorter of the two, when the speed of the wave appropriate to its wave features is approached, will experience great wave-making resistance, and will therefore probably experience greater total resistance than the longer ship. But it is certain that at low speeds when the wave-making resistance of both ships is practically *nil*, the shorter ship will make the least resistance, because the long and narrow one has the largest area of skin, and will therefore have the greatest surface-friction resistance. Judging, however, by the midship section theory, we should have erroneously concluded that the short and broad ship

would make the greatest resistance of the two at all speeds.

Next let us take the two ships, whose water-lines are shown in Fig. 20. It may be seen that the one shown in dotted lines has the same length, and the same sharpness of ends as the other, but is filled out amidships to a larger cross section. On the midship section theory, this one would clearly have the greatest resistance of the two. Nevertheless, in the trial of two models of those lines it appeared that at the higher speeds the form with the largest cross

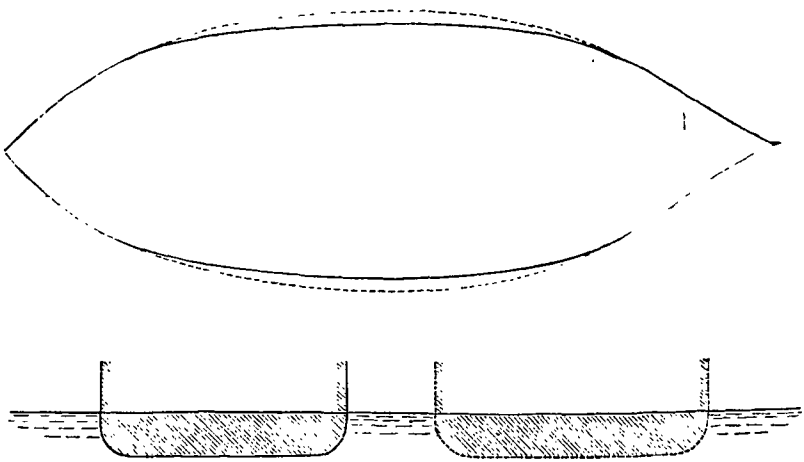


FIG. 20.

section made considerably the least resistance. The explanation of this lies of course in the fact that the addition amidships, though increasing the displacement, forms a prolongation of the wave features of the two ends, and thus lessens the wave-making resistance.

In conclusion, let me again insist, and with the greatest urgency, on the hopeless futility of any attempt to theorise on goodness of form in ships, except under the strong and entirely new light which the doctrine of stream-lines throws on it.

It is, I repeat, a simple fact that the whole framework

of thought by which the search for improved forms is commonly directed, consists of ideas which, if the doctrine of stream-lines is true, are absolutely delusive and misleading. And real improvements are not seldom attributed to the guidance of those very ideas which I am characterising as delusive, while in reality those improvements are the fruit of painstaking, but incorrectly rationalised, experience.

I am but insisting on views which the highest mathematicians of the day have established irrefutably; and my work has been to appreciate and adapt these views when presented to me.[1]

No one is more alive than myself to the plausibility of the unsound views against which I am contending; but it is for the very reason that they are so plausible that it is necessary to protest against them so earnestly; and I hope that in protesting thus, I shall not be regarded as assuming too dogmatic a tone.

In truth, it is a protest of scepticism, not of dogmatism; for I do not profess to direct any one how to find his way straight to the form of least resistance. For the present we can but feel our way cautiously towards it by careful trials, using only the improved ideas which the stream-line theory supplies, as safeguards against attributing this or that result to irrelevant, or rather, non-existing causes.

[1] I cannot pretend to frame a list of the many eminent mathematicians who originated or perfected the stream-line theory; but I must name from amongst them, Professor Rankine, Sir William Thomson, and Professor Stokes, in order to express my personal indebtedness to them for information and explanations, to which chiefly (however imperfectly utilised) I owe such elementary knowledge of the subject as alone I possess.

THE BATHOMETER.

BY DR. SIEMENS.

LADIES and Gentlemen,—I have been asked by the Department to give a description of an instrument that I have placed in this interesting Loan Collection, the name of which is Bathometer. The name is derived from *bathos* the depth, its purport being to measure the depth of the sea without a sounding-line. It is as long ago as 1859 that my attention was first drawn to this subject. Being professionally connected with submarine telegraphy and the establishment of submarine cables, I was struck with the great inconvenience arising through the want of knowledge of the depth below the ship which is engaged in the operation of cable laying, and as it is always interesting to know the origin of an idea, I think I may shortly point out to you the nature of the difficulty which really suggested this idea. In laying a submarine cable, the cable passes from the tank of the ship over a stern pulley and over a dynamometer into the sea. The cable would run out over this stern pulley with an indefinite velocity, unless it were retained by the dynamometer, and the amount of retaining force necessary to prevent the cable either from running out too quickly or from being laid too tight at the bottom is represented by the weight of the cable hanging from the ship vertically down to the bottom. If the depth is a mile and the weight of the cable in sea water is one ton per mile, the retaining force which has to be applied to the dynamometer must be exactly one ton. If less than one ton is applied the cable will run out in undue proportion, and if more is applied it will be stretched tight upon

the bottom and will be incapable therefore of following the sinuosities of the ground. This proposition is proved mathematically, that the retaining force must be equal to the weight of cable. But how are we to ascertain the depth of the sea so as to prevent undue loss of cable? It might be said that the sea should be sounded beforehand, but in laying a cable across the sea, say across the Atlantic, it is not always easy to know where the currents interfere with the path of the ship through the water, and if the weather is at all unfavourable no astronomical observations can be taken, and the actual position of the ship may be very different from the assumed one, and consequently the actual depth, however carefully the sea may have been sounded, may be very different from what it is supposed to be. Hence the difficulty and the importance of knowing the depth of the water. It occurred to me whether it was not possible to keep a running record of the depths of the sea below the ship while the ship was moving. Of course a sounding-line was out of the question, because it takes several hours to let a lead sink down to the bottom of a deep sea; but was it not possible to take advantage of the inferior specific gravity of the water itself, to gain an indication of the depth? This thought occupied my mind, and I constructed an instrument which I thought would give me an indication of the variations in the total gravitation of the earth as the ship passed over the surface. This instrument, however, was not perfect. I had to deal with extremely slight variations in the total attraction, and an instrument to indicate such slight variations, notwithstanding the disturbing influences of the motion of the ship, variations of temperature and variations in the pressure of the atmospheric column, was a problem of some difficulty; and after having tried an instrument and obtained certain indications of success I abandoned the matter until recent events (the occasion of laying the cable across the Atlantic) recalled to my mind the great practical importance which such an instrument would be, not only for the cable layer, but for the navigator generally. But before we can consider the construction of such an instrument we shall have to look to the general question involved.

The law of gravitation, as you all know, was first developed by Newton. Newton's mode of viewing the question I

will endeavour to explain in a few words. Suppose this circle to represent the earth, which for simplicity's sake I will suppose to be of uniform density throughout. Newton divided the earth into concentric shells, and he proved that the total gravitation of each shell would be equal to the mass of the shell concentrated in the geometrical centre of the earth. The sum of all the shells would therefore give the total gravitation of the earth. That being so, it is easy to conceive that in going away from the surface of the earth, ascending over it or descending below it, would materially affect the result of the total gravitation; but it does not follow from this mode of viewing the question that moving along the surface of the earth there would be any variation. The centre of the earth would remain at the same distance, and this being what is called the centre of gravity of the earth, the total gravitation would, it might appear, remain the same. Newton proved that the total gravitation of the earth must vary as you approach from the equator towards the poles. He proved mathematically and indicated the means of ascertaining practically that the poles must be depressed, and this depression would bring the poles nearer to the centre, and therefore at the poles there would be a greater total gravitation. But there is another reason which Newton also pointed out why gravitation should be less towards the equator than towards the pole, and that is the centrifugal force generated by the rotation of the earth. Both these causes—the depression of the poles and the rotation of the earth—tend to diminish gravitation towards the equator, and the ratio of increase of the total gravitation towards the poles proceeds in the ratio expressed by the square of the sine of the latitude; therefore in looking at the state of science with regard to gravitation I did not find much help towards the solution of my problem, and I found it necessary to try another mode of viewing the same. There can be no doubt about this, that the total gravitation of the earth is composed of the gravitation of each portion of it.

Then, in order to calculate the amount of variation that will be produced in the total attraction of the earth, by a given depth of water below the attracted point P, a line is drawn from that point to the centre of the earth, and the same is divided into an unlimited number of indefinitely

thin slices by planes perpendicular to that line. Each slice is composed of concentric rings of the sectional area

$$\mathrm{dh} \,.\, \mathrm{dx} = \frac{\mathrm{dh} \,.\, \mathrm{z} \,.\, \mathrm{d}\, \alpha}{\cos \alpha}$$

and of the capacity

$$\frac{2\,\pi \,.\, \mathrm{z} \,.\, \sin \alpha \,.\, \mathrm{dh} \,.\, \mathrm{z} \,.\, \mathrm{d}\, \alpha}{\cos \alpha},$$

and the differential of the attraction is

$$\mathrm{d} \,.\, \mathrm{d} \,.\, \mathrm{A}' = \frac{2\,\pi \,.\, \mathrm{z} \,.\, \sin \alpha \,.\, \mathrm{dh} \,.\, \mathrm{z} \,.\, \mathrm{d}\, \alpha \,.\, \cos \alpha}{\mathrm{z}^{2} \,.\, \cos \alpha}$$

$$= 2\,\pi \,.\, \mathrm{d}\,\lambda \,.\, \sin \alpha \,.\, \mathrm{d}\, \alpha,$$

which, integrated between the limits h and o and α and o, is

$$\int_{o}^{h} \int_{o}^{\alpha} 2\,\pi \,.\, \mathrm{dh} \,.\, \sin \alpha \,.\, \mathrm{d}\, \alpha = 2\,\pi \,.\, \mathrm{h}\,(1 - \frac{2}{3}\sqrt{\frac{\mathrm{h}}{2\,\mathrm{R}}}) = \mathrm{A}'\,(1)$$

as the total attractive force exercised by the uppermost portion of the earth to the depth h.

For small values of h,

$$\mathrm{A}' = 2\,\pi \,.\, \mathrm{h}\ (2).$$

Substituting 2R for h in (1), there is obtained

$$\mathrm{A} = \frac{4}{3}\,\mathrm{R}\,\pi\ (3).$$

The proportion between the upper segment and the whole earth is

$$\mathrm{A}' : \mathrm{A} = 2\,\pi\,\mathrm{h} : \frac{4}{3}\,\mathrm{R}\,\pi,$$

$$= \mathrm{h} : \frac{2}{3}\,\mathrm{R}.$$

If sea-water were without weight, the attraction at P would be diminished in the above ratio of depth of water of $\frac{2}{3}$ R, but the real influence of the depth, taking the specific gravity of water into account, is very nearly as the depth to the earth's radius.

If this calculation is correct, it follows that the total gravitation of the earth will diminish in passing over the surface of the ocean in the exact proportion of the depth of the sea to the radius of the earth, and as the radius

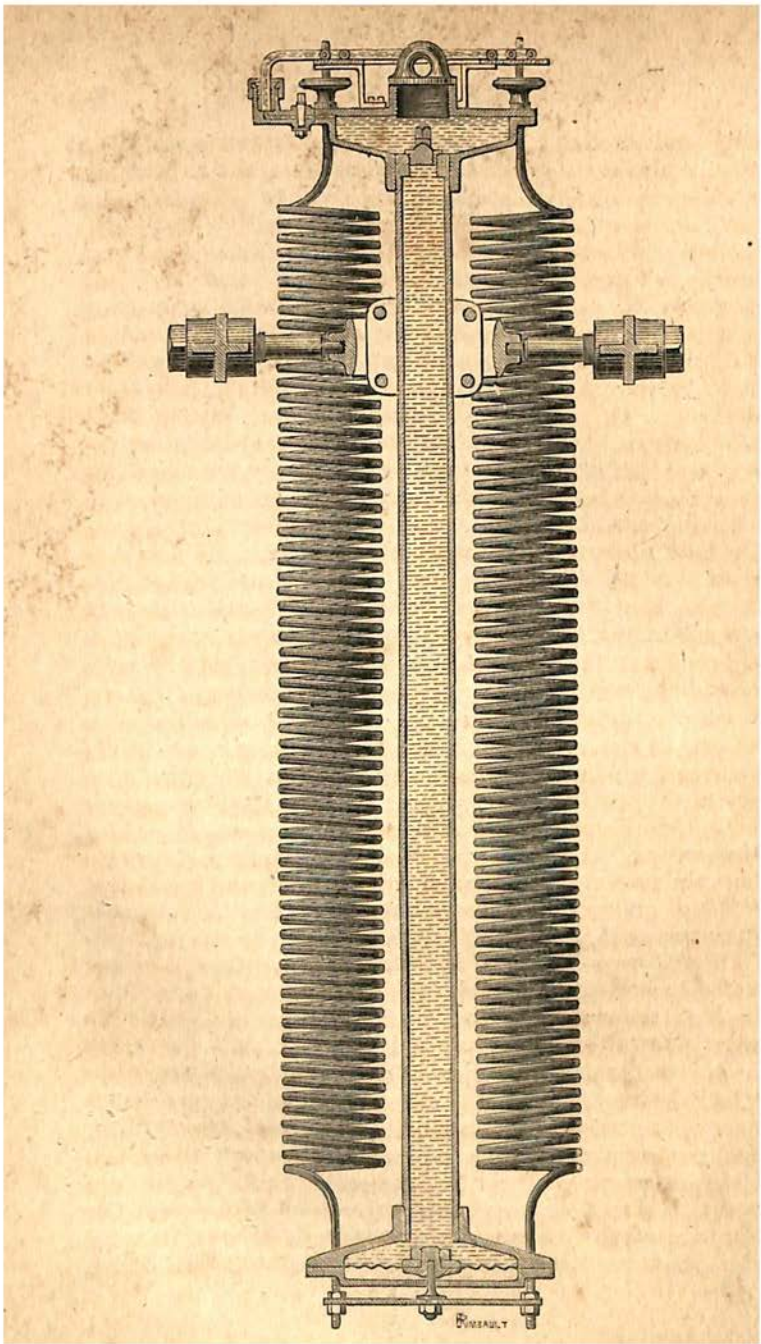
of the earth may be taken roughly at 3,950 miles, the depth of one mile of sea-water will diminish the total gravitation by $\frac{1}{3900}$ part, or a fathom depth of sea-water will diminish the total gravitation by $\frac{1}{3,600,000}$ part. This calculation is of importance as showing that the decrease of gravitation must be proportionate to the depth; but this result is of course influenced, as I have already mentioned, by latitude and by other disturbing causes, which have to be allowed for in dealing with these instruments, unless they are corrected in the instrument itself, and that is a subject which we shall have to consider.

Besides the disturbance in the total gravitation due to latitude, there is one which was first referred to by Sir George Airy, and which Professor Stokes has assigned to its true cause, namely, that the total attraction of the earth is always greater on islands than on the mainland. On the shore of the mainland the total attraction of the earth in the same latitude is less than on the shore of an island, and Professor Stokes proves that this is owing to the attraction of the mass of the continent itself. Its mass lying above the general surface of the earth exercises naturally a negative influence upon the total gravitation, and it follows that the total gravitation at the sea-level must be less in proximity to large continents than it would be on the shore of a small island. It has also been proved by Archdeacon Pratt and others that the attraction of mainlands upon the water itself must draw the water towards this mainland, and that hence the water-level near shores must stand higher than the water-level at a greater distance from the shore. In other words, a continent draws the water towards it and raises the sea-level near shore. I have received a paper from Dr. Hann which deals with the same subject, and he goes farther than those who dealt with it before him, and maintains that the total attraction of the earth owing to this continental local attraction would be greater at mid-ocean than near shores—that is to say, that the depression of the water-level would be such that the gravitation on the surface of the ocean would actually be greater than the total attraction on the sea-shore. If Dr. Hann is right I am wrong, because the instrument which I have the honour of bringing before you is based upon an exactly opposite

hypothesis, and I doubt that Dr. Hann takes the attraction too much in the sense of astronomical attraction rather than in the sense of local attraction. Here I would refer to an experiment which we all know and can easily make. Any large substance falls towards the centre of the earth. If we throw a weight against a wall it falls vertically down to the earth, but if we throw a drop of water against the wall it does not fall towards the earth—it adheres to the wall; and if we throw a small particle of sand against the wall it does not fall, it adheres to the wall. This is explained by the adhesion. Adhesion is only another form of attraction. It is attraction at very small distances; and for the grain of dust hanging against the wall, the centre of attraction of the whole earth may be thought of as concentrated in the wall, and as falling within the substance of the wall, because this local attraction is so great owing to its vicinity to the particle that it overcomes actually the greater attraction of the total earth. And so a small insect mounting up a wall would probably be influenced more by the attraction of the wall, if it is a massive one, than by the total gravitation of the earth; and it must always be borne in mind that the attraction of the substance nearer at hand exercises an influence which is greater than the mass at a distance, in the proportion of the square of that distance, which is a very large proportion.

The next problem was the construction of such an instrument, and this presented considerable difficulties, because we have to deal with exceedingly small variations of total gravitation, and we have to deal with disturbing influences which should be eliminated or allowed for.

I will now describe the instrument. One is placed before you of the size which I have constructed at present, and an enlarged drawing is placed upon the wall. The total gravitation of the earth is represented by a column of mercury which rests upon a thin diaphragm of steel plate. This diaphragm of steel is embossed in such a way that its centre can move within a small range freely up and down under the influence of a column of mercury without encountering any frictional resistance. The mercury column ends again in a cup; and the whole system, the pipe and the cup, are filled with mercury up to the line shown in red on the diagram. The space above the mercury is filled up



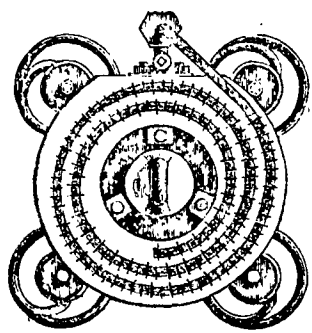
with another liquid of less density, with water or spirits of wine, and this other liquid terminates in a spiral tube laid upon a scale at the top of the instrument. The centre of the diaphragm which supports the column of mercury is carried by two springs, or by four as in the case before you, which are made of carefully-tempered steel and are stretched out to such a degree that the elastic pressure exercised by these springs exactly balances the dead weight of the column of mercury resting upon this diaphragm, the result being that the diaphragm retains its horizontal position. This column of mercury presses with a force resulting from the area of the diaphragm multiplied by the height of the column, and it will be seen at once that this force is considerable. This diaphragm is 90 millimetres in diameter, and the height of the column is 600 millimetres. The total force of gravitation resisted by this diaphragm is equal to about 120 pounds, and therefore any variation in the total gravitation would be a variation in the weight of over a hundredweight, and the $\frac{1}{8.500.000}$ part of this amount still a measurable quantity. The central part of this column has been reduced. Instead of having the column of mercury from end to end in a parallel cylinder, it is reduced in the middle to a much smaller area, and this is not done without an object. If the mercury stood in a column of equal diameter throughout, and if the area of this column remained the same under all circumstances of temperature, it would be easily seen that the potential or the total pressure of this column of mercury upon the diaphragm would be always the same, whatever the temperature or the state of expansion by temperature of the mercury might be, because in proportion as the density of mercury is diminished through dilatation, the height of the column would increase and the one would exactly compensate the other. But we have to consider that the balancing force to the force of gravity is a variable force, the elastic force of the spring; and I had to determine by careful experiment what is the amount of variation which this elastic force undergoes for a given variation of temperature. Wertheim in Germany had already investigated this subject, but hardly to the extent necessary to guide me in the construction of this instrument. The result of my experiments, which on the whole agree with those of Wertheim, proves

that the elastic force of the spring diminishes with an increase of temperature in an arithmetical proportion, and in a proportion which is inferior to the dilatation of mercury. Therefore by diminishing the amount of mercury between the diaphragm and the surface I arrive at a point where the total variation in the potential force of this column of mercury varies by temperature in the same proportion as the spring varies also; and if these proportions are properly adjusted the instrument before you will be parathermal or remain uninfluenced by variations of temperature. That was a very important point to establish, because if the instrument had to be maintained at a uniform temperature, or if calculations had to be made to adjust it for temperature, those variations would very likely overbalance and swamp the indications of the instrument due to depth, and so would have rendered it useless. It was chiefly owing to this difficulty that I did not succeed sufficiently well in my first attempt.

The atmospheric pressure which determines the height of the mercury in a barometer exercises no influence on this instrument, because it is exerted upon the lower side of this diaphragm as well as upon the upper surface of the mercury, and assuming the pressure to be the same at each level or proportionately the same, there will be no influence; but there is a slight influence due to variations in the atmospheric density as distinct from atmospheric pressure. The weight of this mass of mercury in the tube is only in as much as the mercury is heavier than atmospheric air. If the difference of density between mercury and air were small, then any increase in atmospheric density would not only influence the pressure of the mercury on this diaphragm very materially, but enormous as the difference is between the density of mercury and air, yet a variation in atmospheric density exercises an influence upon the potential of this column of mercury, and this variation will be sufficient to influence the delicate indications which we expect from it. This has been eliminated by suspending the instrument in an air-tight casing. It is now raised out of its casing for you to see it, but when in use it descends into the case and there swings freely on its gimbals, and although there is the atmospheric pressure inside the casing of average density, yet, as this casing is

closed by means of a disc of plate glass, the variations in density do not affect the quantity of air enclosed within its casing, and we thus get rid entirely of the disturbing effects which would arise from this cause.

At the bottom of the figure is shown an apparatus by which I used to take the readings of the instrument, which consisted of a micrometer screw to which an electric contact piece was attached, and in screwing this micrometer screw until contact was obtained, which was evidenced by the ringing of a small electric bell, I could ascertain the exact amount by which the diaphragm was being depressed or raised by the natural balance of the two forces—the



elastic force and the gravity force; but this mode of reading was inconvenient, it required great care on the part of the observer and did not furnish a ready indication of the state of gravitation or of the depth of the sea below the instrument without first going through this operation of making the contact. The arrangement which I have now adopted is of a much simpler kind. The mercury ascends into the cup up to nearly its upper surface, the remaining space is filled up by water, and the water ascending into the spiral tube upon the scale carries with it an air-bubble, and it is the position of the air-bubble in the tube which gives me the indications of the state of the instrument. It will be readily seen that the amount of depression in the cup will cause a proportionate motion of the bubble in the cylindrical tube, and hence the amount of motion of

the bubble upon the scale furnishes a correct indication of the amount of action due to the increased or diminished force of gravitation on the instrument—all other conditions being equal. Another cause of disturbance on the instrument is due to the pumping action of the ship. A ship in going over the ocean, as we all know—some of us from unpleasant experience—has an upward and downward motion; and disagreeable as it is to us, it is still more disagreeable to this instrument, unless it is so arranged as to represent a good sailor. The mode in which this difficulty has been dealt with consists in stopping up the upper end of this tube, leaving only a very small cylindrical perforation for the flow of the mercury in one direction or the other. The diameter of this hole is only about one-tenth of a millimetre, and in that opening we still find a pumping action equal to about 10 millimetres on this scale—that is to say, when the ship goes up and down this air-bubble moves 10 millimetres to and fro, and we have to get our reading by taking the mean between those two positions. If this motion was much greater than 10 or 15 millimetres, it would be difficult or impossible to get accurate readings while the ship was in motion, but it is not so difficult to get the true reading with a variation of so small an amount.

It was important to ascertain what the influence of latitude was upon this instrument in order to get the true comparison between the influence of latitude and the influence of depth, and here I have met with a result which requires further explanation. The influence of this instrument moving over a varying depth of the sea has been well ascertained in sending it several times across the Atlantic ocean. I have also tried to get the influence of latitude determined by sending it to Brighton, back to London, and up to Scarborough; and I have observed that while the influence of the depth of the sea is rather in excess of what our calculations had given, the influence of latitude is less than calculation would imply; and it remains to be seen how these discrepancies can be explained. But so much is certain, that according to all observers, the gravitation on the sea-shore is influenced by a variety of causes, such as the attraction of continents, the density of the mass immediately below; and I have no doubt that further observations will clear up this apparent discrepancy. I

have also taken the instrument up a certain height—to the clock-tower of Westminster for instance—and have observed an influence which was nearly equal to what calculation implied, although in taking such an instrument up the clock-tower it is impossible to avoid some disturbing influences which render the result of a few observations less reliable than might be wished.

I will now refer to the results of actual observations taken several times across from this country to America, on board the steam-ship *Faraday*. The instrument was suspended near midships, and observed from time to time while the sounding-line was let down to obtain the corresponding actual measurement of the depth. The results by the bathometer and by Sir William Thomson's sounding-wire are placed in parallel columns, and the differences are not great. There is enough to show that the instrument gives indications which in all cases approach very nearly to the actual results of measurement. We find in all these cases that the instrument and the sounding-line agreed within something like 5 per cent., and I thought that a very satisfactory result considering the instrument is a new one and defective in many respects, and as we are all new in the use of the instrument. There is also another set of observations which have been sent me from Nova Scotia, lately taken by my nephew Mr. Alexander Siemens, and they show the same close agreement between the two modes of measuring in smaller depths. I should here observe that it would be impossible to obtain exactly similar results in reading this instrument, and in measuring by means of a sounding-line, because you really measure different quantities. The sounding-line gives the depth immediately below the ship, whereas this instrument gives the average depth over a certain area. Now this average depth over a certain area will coincide with the actual depth below the instrument if the sea-bottom is on a general slope. The greater attraction at the rise of the slope will be balanced by the lesser attraction where it descends, but if the sea-bottom is irregular—if there are rocks projecting from the bottom—the sounding-line may accidentally come upon a rock and give a small depth, or may accidentally fall beside the rock and give a greater depth; whereas this instrument would give the same depth, whether it was exactly over the

rock or beside it. And in one way I consider that the depths given by this instrument are more valuable than the others. You do not want to know what is the accidental depth—if there is a big stone just under the ship; but you want to know the depth of the ocean, and this gives it more correctly than a sounding-line.

I have now endeavoured to give you a short account of this instrument, and before concluding I would add a few words regarding its possible application. I have at the outset observed that my attention was directed to the matter from the necessity I felt for getting even approximate indications of the depth below a cable-laying ship; but would not such an instrument be also of considerable use on board an ordinary ship? Suppose that we knew the configuration of the bottom of an ocean—say of the Atlantic—more accurately than we do at present, and that a ship with such an instrument on board were to start from America to come to this country; suppose gales supervened and that the state of the clouds made astronomical observations impossible, and this continued to be the case for days together, such an instrument would give the navigator at all times an account of the depth below him, and if he traced his line over the ocean according to his dead-reckoning, coupled with observations of this instrument, it would be almost impossible to lay down two lines that would answer to the two observations. The variations of depth together with the direction of the compass and the dead-reckoning would give him with very considerable precision the position in which the ship was at any moment, because if he assumed that gales had driven him away from his course, it would be impossible to find in any other latitude the soundings correspond with the indication of the instrument; and more particularly in approaching the opposite shore the navigator would obtain from such an instrument a very fair indication, not only of his distance from shallower waters, but, through the ratio of variations shown by the instrument, the exact position at which he approached the land. It is on that account that I believe this subject is well worth following further in order to make the instrument perhaps more compact and more perfect in every respect, so as to bring it within the compass of the navigator; and if by this means the safety of navigation

can be increased, however much or however little, I think it is an object well worthy of effort.

If I may be allowed a few minutes more, I will refer to a corresponding instrument for measuring horizontal attractions. There is an imperfect diagram suspended here and the instrument is down stairs. The object is to measure local attraction in a horizontal direction, and it serves to illustrate the principles upon which the bathometer is based. There are two bulbs connected longitudinally by means of an iron tube, and the iron tube and the bulbs up to their mid-centre are filled with mercury. The mercury naturally will assume a horizontal position in the two bulbs, except when influenced by a horizontal force. Such a force would have a tendency to draw the mercury in the tube towards it, and this attraction exercised upon the column of mercury would cause it to rise in one bulb, and to descend correspondingly in the other. This difference of level could not be observed by any instrument; but the upper portion of these bulbs is filled with spirits of wine, and the spaces filled with the alcohol communicate by a glass tube with a single air-bubble so adjusted as to occupy any position in this tube on the scale. It follows then that any horizontal attraction which may affect the level of the mercury, in however small a degree, will force some alcohol through the tube, and carry this air-bubble with it over the scale to an extent exactly proportional to the horizontal attraction exercised; and if the cross-section or area of the tube is made 30,000 times less than the area of the bulb, it follows that a rise of mercury of $\frac{1}{30,000}$ of an inch in the bulb would cause the air-bubble to move an inch; and with such power of multiplying the scale we have succeeded in obtaining indications even of the weight of a single person passing from one side of the instrument to the other. It thus renders sensible the effect of our own weight in proportion to the enormous weight of the earth. It might even be supposed that a weighing-machine could be instituted, which being firmly mounted on a solid foundation, would give you indications of any passing weight in its vicinity; but another application which may be a useful one, is to measure the effects of diurnal variations and the horizontal attraction exercised by the moon and the sun,—the force which produces the

tidal wave. Sir William Thomson takes some interest in this instrument, and he will I hope on his return from America take it up, with the view of getting some useful results such as he is best able to attain.

BATHOMETER READINGS TAKEN ON BOARD SS. *FARADAY.*

October and November, 1875.			March and April, 1876.		
Bathometer.	Sounding.	Difference.	Bathometer.	Sounding.	Difference.
201	197	+ 4	90	90	
99	100	— 1	94	93	+ 1
63	54	+ 9	95	94	+ 1
82	82		107	101	+ 6
218	204	+ 14	105	105	
78	69	+ 9			
56	54	+ 2	64	64	
55	54	+ 1	64	61	+ 3
50	56	— 6	56	53	+ 3
47	54	— 7	66	68	— 2
50	58	— 8	29	27	+ 2
66	69	— 3	43	38	+ 5
82	73	+ 9			
56	47	+ 9			
49	46	+ 3			
80	69	+ 11			
111	100	+ 11			
215	200	+ 15			
69	64	+ 5			
80	80				
86	86				
68	76	— 8			
388	353	+ 25			
799	698	+ 101			
607	503	+ 104			
2789	2516	+ 273			
2388	2320	+ 68			
1907	1861	+ 46			
1615	1700	— 85			

INSTRUMENTS FOR EXPERIMENTS ON SOUND.

LECTURE I.

It is my intention to speak this morning on certain modes of eliciting, reinforcing, or distributing sound. Of course we cannot go into every such method—it would be impossible within the time allotted. What, therefore, I propose to consider are the more scientific modes, which are not exactly musical, but such as are employed for experimental and computative purposes. The excellent classification given by Professor Clerk-Maxwell in the handbook of this Exhibition will be my guide. He speaks of vibrations and of waves; taking first amongst such vibrations the physical aspect of acoustics.

Vibrations and Waves.

PHYSICAL ASPECT OF ACOUSTICS.

1. Sources. Vibrations of various bodies.
   Air.—Organ-pipes, Resonators, and other wind instruments.
   Reed instruments.
   The Siren.

Strings	.	Harp, &c.
Membranes	.	Drum, &c.
Plates	.	Gong, &c.
Rods	.	Tuning-fork, &c.

2. Distributors. Air . Speaking-tubes, Stethoscopes, &c.
   Wood . Sounding-rods.
   Metal . Wires.
3. Pugging of floors, &c.
4. Reservoirs. Resonators, Organ-pipes, Sounding-boards.
5. Dampers of Pianofortes.
6. Regulators. Organ Swell.
7. Detectors. The Ear; Sensitive-flames, Membranes, Phonautographs, &c.
8. Tuning-forks, Pitch-pipes, and musical scales.

Now of these we shall consider to-day principally monochords, tuning-forks and sirens, under the former head, that of eliciting; in the second place, that of reinforcing and distributing, resonators of various kinds, and telephones. The latter perhaps might have been more distinctly specified in the title of the lecture, as what Professor Clerk-Maxwell terms distributors. Before, however, adverting to the means of eliciting sound, we can hardly avoid mentioning something as to vibration in general. We find it proceeding from ordinary pendular vibration up to the most delicate vibration of ether, on which rests the fundamental hypothesis of light, and we can observe this vibration in various ways.

A very ingenious instrument is here contributed from abroad which enables you to combine one or more harmonic motions. The string is strained between two elastic terminals, both of which by means of electro-magnets can be set into oscillatory motion. By putting the first alone into motion we get single vibration; by joining and coupling up with it that at the other end, which can be rotated round its axis, we can combine another harmonic motion, either in the same direction making complex vibrations, or at right angles, or indeed at any given angle; thus compounding it into various regular figures, ellipses, circles, and other curves such as were produced by Lissajous.

Taking this end of the vibrating string first, I bring the battery in connection; then straining the string to the right tension you will see very distinctly that it is vibrating in two segments forming a node at the middle point. The string

is studded with small white points to enable it to be seen. Now if I put on the second magnet at the other end, turning it into the same circuit, and combine the vibrations of those two, the figure becomes much more complicated. There are three or four ventral segments, two different variations being thrown into the string at the same time. Then compound them still more by turning the second magnet round upon its axis and setting them in two rectangular directions. A beautiful elliptical figure is formed thus by the vibrations in a horizontal direction at one end and in a vertical direction at the other. This apparatus shows the vibration of a string; I will next demonstrate another means of combining the vibrations of reeds. This is a very pretty contrivance, contributed by Mr. Pichler, who kindly comes to assist me in exhibiting it. The instrument consists of a wind-chest with means of blowing; above it there are two reeds, one fixed in the vertical and the other in a horizontal position; by shifting the bearing of one reed it can be made to double its length, so that the vibrations shall be to each other in any ratio from that of unison down to an octave below. We can pass through all the intermediate figures. On each reed is placed a small mirror, and here is a limelight, the beam from which falls first on the mirror of the upper reed, it returns and is reflected on the second, whence it is thrown on the screen. Whilst the mirrors are still, the spot of light on the screen is motionless; when we set them vibrating, the one in a vertical direction gives a vertical line of light, when the other in a horizontal direction is added they combine their two harmonic motions and give beautiful curves, which I have already named. The circle denotes unison; the varied figures are produced by varying phases and velocities at the two reeds.

Strings were the earliest sources of sound and were the first used for acoustical experiments; indeed, in ancient Greek times determinations were made of the laws of sound from strings, which are generally attributed to Pythagoras, though Mr. Chappell in his erudite work concludes that Pythagoras's information came originally from Egypt or even can be traced back to Babylon. At any rate, by Euclid's time a very perfect knowledge of the laws of strings had been attained. Euclid wrote a work called *Sectio Canonis*, the division of the string or monochord by which all these ratios are obtained.

From this time we have a long gap until we come to the age of Galileo; but even in Greek times the simple ratios and

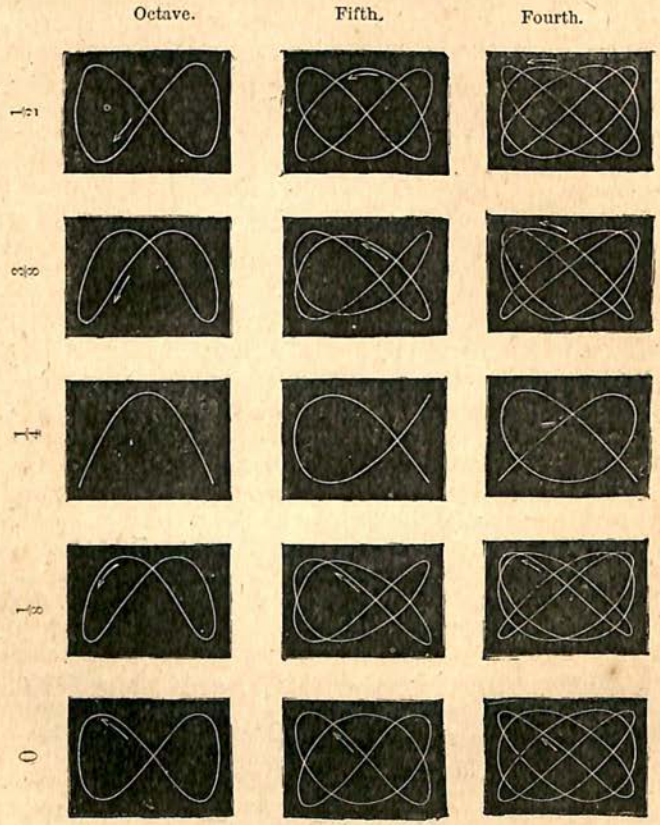


FIG. 1.—Optical curves. The octave, fourth, and fifth.

parts of the string were well known. I have placed these on a diagram there just as a reminder, because we shall have to speak again to-morrow of a different form of numerical calculation not involving ratios; to-day I wish to bring before you the ratios and nothing more. You can see how these

ratios would have produced by a geometrical method those beautiful figures projected on the screen. The principal ratios are :—

the octave	2 to 1
fifth	3 „ 2
fourth	4 „ 3
major third	5 „ 4
minor third	6 „ 5
major tone	9 „ 8
minor tone	10 „ 9
major semitone . . .	16 „ 15

I have also written down in small figures below (I do not wish to confuse you with them to-day, as I shall have to say more about it to-morrow), the ratio of the comma, a computational interval, 81 to 80 : this at present I will pass over.

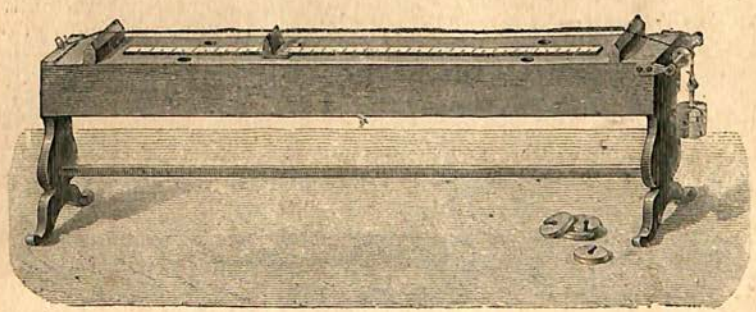


Fig. 2.—Monochord.

I will now proceed to show the monochord and the different ways of using it. There is in the South Kensington Museum an old monochord by Broderip and Longman, which was intended for persons to tune their harpsichords. It consists of a small string with definite marks placed under it to which you can set a fret or stop to check the vibrations. There is also a little harpsichord "jack" by the side, a piece of quill, the predecessor of the pianoforte hammer, by which the string was plucked. Here I have a monochord a metre long divided into decimetres and centimetres with

bridges at the end and pegs by which these strings can be strained, either by twisting wrest-pins, or by adding weights. Now adding weights is very much the better plan for experimental purposes, and it was the plan employed by the father of all enharmonic instruments, Col. Perronet Thompson, whose organ is exhibited in the Loan Exhibition and of which I propose to speak to-morrow. Weights have a great advantage in a monochord, because the raising of the note of the string is not in simple ratio to the weights you add, but in that of their square. For instance, if I load the string with a certain weight, two of these separate blocks, and then add two more, I do not raise it to its octave or anything like it. The advantage is that accidental variation in the stretching weight causes only a comparatively small error. A monochord of this kind was used by Perronet Thompson for tuning the pipes of his enharmonic organ. He chose stouter wire and very heavy weights, sometimes more than 250 lbs. since the best steel wire will stand that weight; against the notes so produced he cut the pipes of his organ the right length by getting unison.

Some considerable time ago, an ingenious gentleman of the name of Griesbach carried this contrivance of the monochord still further. He not only measured the ratios of tones but contrived a method of drawing and printing them; this instrument we have in the Exhibition. We have also several smaller monochords of Mr. Griesbach's. Here is one with a scale of aliquot parts very elaborately made to measure. Here is a string with fixed points upon it by which the tempered scale of the octave can be accurately obtained. It is an independent reproduction of Broderip's instrument. I wish to call your attention particularly to this large instrument. It has a double-bass string, stretched along this bar of wood, with a sounding-box, and there is a means of tightening it by a screw. Here is a very ingenious rotating bow; somewhat damaged by time, but of which the principle can still be seen. A piece of vulcanized Indiarubber is covered with horsehair and then, being passed by means of rollers over the string, it gets rid of the great difficulty experienced in using strings for tuning, namely, the evanescence of their tone. It is curious to see later discoveries anticipated by an ingenious man whose labours have been somewhat overlooked. He did more; here again anticipating modern

instruments, he placed a paper and a tracing-point, with blackened tissue behind, so that when the string vibrated the point pressed against the paper and produced curves. You thus can not only measure the string and get the ratios, but you secure permanent vibrations by means of the rotating bow, and you can also print them off on a strip of paper which travels slowly in front by means of the hand, or as here, by means of a weight, so as to bring it gradually past the vibrator.

There are other modes of exciting strings besides striking them, such as by bowing; of course many instruments act in this way. For observations of an acoustical character bowing is not so good; it is apt to produce partial vibrations. We may also excite strings by the impact of the air. There are specimens of struck strings in the pianoforte actions which are exhibited. Bowing you are all probably familiar with. The impact of air, if not entirely a new discovery, has only lately been put to practical use. I do not propose to go into it to-day, because my friend Mr. Baillie Hamilton will deliver a separate lecture upon what he terms "æolian" modes of producing sound, in which the combination of a string with a reed brings out new and beautiful characters of tone. Strings when struck produce many upper partial tones, according to the place where they are struck, according to the nature of the stroke, and according to the density, rigidity, and elasticity of the string. I must refer you to Helmholtz's great work for further details on that point; only noticing what pianoforte-makers have discovered by experience, and what Helmholtz has explained theoretically, that if the hammer strike the string in the pianoforte at about one-eighth or one-ninth from one end certain dissonant upper partial tones are excluded and a much finer effect is secured. The second form of bowing the string, as illustrated in violins and other instruments, was examined by Helmholtz by means of what he terms the vibration microscope, an ingenious plan for producing to the eye of a single observer exactly what I have shown you on the screen. He sets a string into vibration, fixing a small grain of some white substance, generally starch, on it, and looks at it through a microscope which, instead of having a fixed object-glass, has the object-glass mounted on the prong of a tuning-fork. That tuning-fork is made to vibrate in

the transverse direction to the string. Here again we have the same composition of harmonic motions which I have already shown you, one instrument deflecting the ray laterally and the other vertically; so you get regular figures, which become steady when unison or concord is going on, but which flicker into innumerable changing lines when dissonance is present. In this way he was enabled to analyse the vibrations of a violin string in motion, and remarked that regular figures, free from jumps, starts, and abrupt changes—smooth vibrations, in fact, such as you saw just now—were more easily obtained from fine old instruments than from raw modern fiddles. This is very curious, because it has always been a great question of doubt and difficulty why old violins produce a so much finer tone than modern ones. I have endeavoured myself to utilize this observation of Helmholtz by rendering the sound-board of the fiddle more homogeneous. Here is an instrument to which the contrivance is applied so as to get the sound transmitted more nearly like that of a fine old instrument. I cannot go fully into the question of tension bars, but I find better effect is produced by putting strengthening bars along the belly of the fiddle, so as to make it more homogeneous without adding materially to its weight. Helmholtz also found that the interior of an old fiddle adds resonance by the body of wind it contains; I have here an old violin, and an old tenor; if we blow into the body as into a wind-chest we can repeat his observation; we can use it, in fact, as a sort of whistle or organ-pipe; of course it gives a rough note, but still you can hear the pitch of it. The tenor is rather clearer, and there is quite the difference of a tone between the two. The result of Helmholtz's experience was that a Straduarius violin gives C, tenors a note lower, and violoncellos generally give F, or G, in the bass.

I proceed next to speak of rods, bars, and tuning-forks, which are only exceptionally used in artistic music; although there is an instrument employed by Mozart in the *Flauto Magico* to imitate the sistrum, with which Papagino is supposed to be gifted, consisting of metal bars which strike a scale of high notes—it is called a *glockenspiel*. This is only an exceptional case to produce a particular effect, but I can show you the character of such notes by means of a steel bar. If I take this bar of cast steel and strike it on one end, you hear first of all rather faintly the fundamental note such as I

get by striking it across, but you hear also intensely high upper partial notes which sound very persistently, so that even in this large room it will be possible to hear an excessively high note above the range of the highest piccolo that ever sounded; and it will continue for several seconds after the blow. If the bar or rod be supported at more than one point it forms the usual harmonicon. We have here two very remarkable instruments of this character; one is on the plan of a musical-box. It is very singular that this should have been contrived so well by a half-savage tribe in Angola, that you can get a perfect scale out of it; the bars of metal are supported at one end on a resonance-box of wood, there are also feathers under them, but they are connected with some fetish superstition. Passable music might have been got out of this. Here is another which I find in the Exhibition, which may be recognised as one of the various attempts at wood harmonicons. These instruments have been formed of all sorts of things, of wood, stone, glass, and metal. A clever little boy was brought forward some years since to play what was called a "xylophone," which consisted of pieces of hard wood; on which he really performed very creditably. Then there was the "rock harmonicon" which I can remember in my early days, and the glass harmonicon you must know very well. The one I show is a wood harmonicon. It is formed by adding resonators of a very ingenious kind to bamboo blocks. One of these has unfortunately been broken in carrying it over, but that same accident enables us to look at the mechanism. The resonators are formed of gourds or calabashes, outside which is put a little ear-trumpet to act the function of the pinna of the human ear, to collect the sound; in the hollow of this I discover a small membrane, a piece of thin material resembling goldbeater's skin, intended to reinforce the sound. Here then is one of the last discoveries of Helmholtz anticipated and utilized in the wildest parts of Africa. We have other more developed forms of this instrument, such as the musical-box, which is excited by mechanism. We have the vibrating bar partially producing the sound in the Jew's-harp, and regulating the vibration of a column of air in the harmonium. The form I wish to speak of to-day is the tuning-fork.

Tuning-forks form a great portion of the experimental apparatus of acoustics. They may be looked upon simply

as double rods vibrating in opposite directions, and thus dispensing with a firm fixture; because one vibration counterbalances the other. A single rod of course transmits its vibration to the support, unless it be very solid, but in a double rod this is not so. When they are struck alone like the bar of steel which I showed you just now, they give very

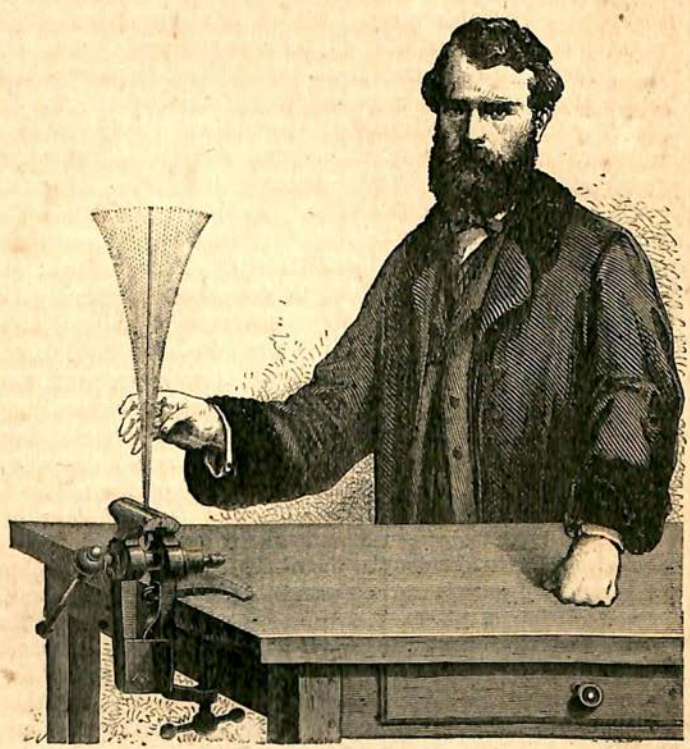


FIG. 3.—Vibrating Bar.

high secondary tones. When I hold this and strike it, you do not hear more than a high "*ping*," though there is here a slight sound of the fundamental note. This we can reinforce till it becomes of considerable value. Tuning-forks are amongst the instruments whose use has extended from sound into other branches of physics, after a pleasant fashion of

reciprocity. They have been employed of late years as a measure of time; their pendular vibrations are so regular, so accurate, and so easily adjusted to any one period of vibration, that they furnish an admirable means of measuring small intervals. Here is a beautiful instrument contributed by the French *Conservatoire des Arts et Mètiers*, in which a tuning-fork has been constructed for that purpose. It has little stiles attached to the prongs, and as it vibrates they touch a piece of blackened paper which runs slowly past them. The tuning-fork makes an undulatory line upon it, which is the harmonic motion as it were unfolded. Another stile beside the first enables you to mark any instant of time; for example, the passage of a star across the wires of a telescope, and to measure the exact period at which this took place by counting the number of pendular vibrations which the tuning-fork has made since a given period, previously marked on the paper. Acoustical instruments have also been found useful even for the measurement of the rapidity of light; the coarser form of vibration serving to measure the quicker and more ethereal. Foucault's beautiful instrument for measuring the rapidity of light is in the Loan Exhibition, and you will find that it is worked by the instrument which I shall speak of presently, namely, a small siren. He found that the best plan to make a mirror rotate at the enormous speed required, was to attach it to a small turbine or siren played by steam at a high pressure; as it rotated more quickly so the note went up. The number of vibrations is easily known from the pitch of the note; and he could thereby say how many times in a second the rapidly rotating mirror was revolving, simply by taking the pitch of the siren which was going round with it.

When tuning-forks are struck alone, as I said, they give a very feeble note, but we can alter this by combining them with some resonator. The usual resonator is a box containing a body of air; but Helmholtz has pointed out that a string can be made to perform the same function. The arrangement of the string is a little elaborate, but everybody knows the plan by which tuning-forks are fastened on a resonance-box, and the moment it touches it, it gives the tone; of that I shall speak again. The weak point musically of tuning-forks is, the very evanescent character of their sound. It is troublesome, the moment you have struck

it, and are fully occupied in tuning your instrument, for the note to fade away and die out. We may partially get over this difficulty by bowing them with a double-bass bow, but the highest tuning-forks are difficult to bow, and the best plan, which has been carried to a great pitch of

Fig. 4.—Tuning-fork on box.

accuracy by Helmholtz, is to excite them by electricity. An intermittent current is made to pass through one prong of the fork by means of a stile and mercury cup, enabling the prong to close the circuit. An electro-magnet pulls by its

attraction on the prong of the fork, breaking contact by so doing; a fresh contact is thus made, and so the fork is kept in permanent vibration. I have here an apparatus which I have made for this purpose (you will excuse my mentioning that much here shown is the work of my own hands). When the magnet is formed, it separates the prong and lifts the stile out of the mercury cup in so doing; the fork is now in vigorous vibration and produces a note, which at present you cannot hear, but by bringing a suitable resonator

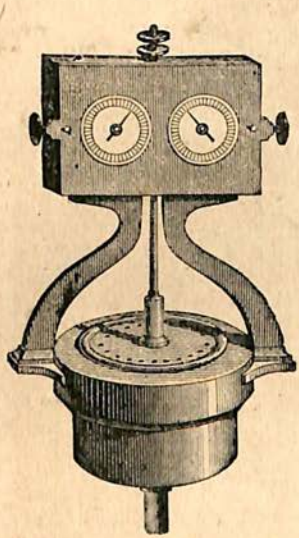


FIG. 5.—Cagniard de la Tour's Siren.

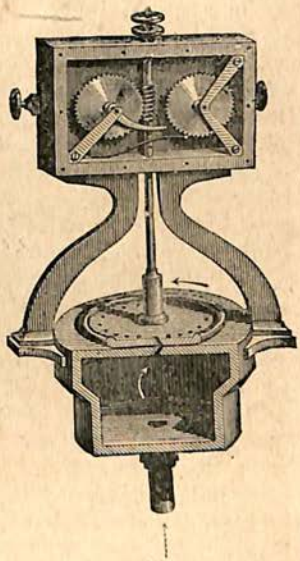


FIG. 6.—Interior view of the Siren.

to it you will hear it distinctly. In that way Helmholtz has been able to keep eight or ten forks all vibrating from one principal fork. Here is one of these principal forks, sent from Paris, having a mercury contact upon it, and there is also a series of secondary forks which have only the electro-magnet and which can be thrown into secondary vibration from this; you can thus reproduce the various vowel sounds which have been explained and demonstrated by Helmholtz.

I have next to speak of sirens. This fanciful name was

given to these instruments by Cagniard de la Tour, because it is said to sound under water. I never heard myself, although I have read in the *Odyssey*, that those charming though dangerous young ladies named Σείρηνες did sing under water, but I believe that is the derivation of the word, and I leave it as I find it. Sirens are entirely unknown as musical instruments, though they have played an important part, not only in

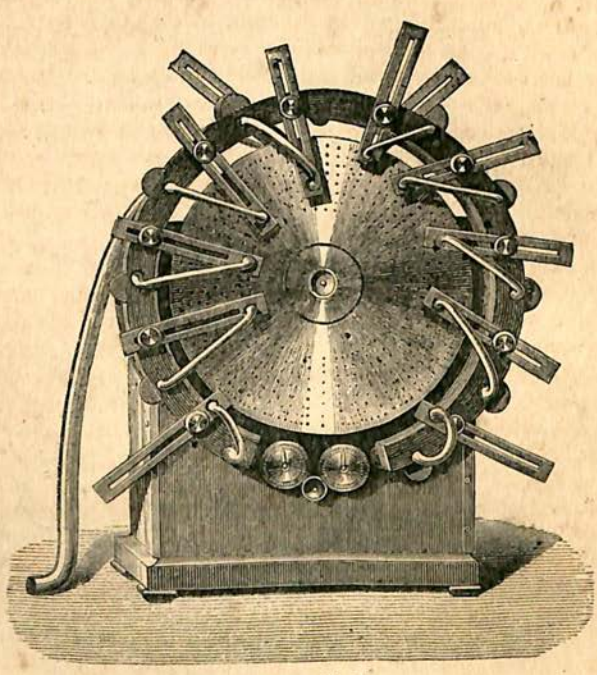


FIG. 7.—Seebeck's Siren.

acoustics, but in the investigation of light; singularly enough they are used for saving life. In going over the Exhibition galleries you will see some enormous steam sirens wherein you can study the arrangements. They are intended to be blown on the coast by means of steam, and to send out to sea a powerful sound warning mariners away from dangerous rocks. The simplest form used in the laboratory is a rotating

disc of cardboard pierced with a number of holes at regular intervals. It is made to rotate in front of a small wind-chest in which there are keys so that a stream of air can be directed against any particular ring of holes. The form adopted by Cagniard de la Tour merely makes these holes vertical, and has a little pipe coming up. A still more perfect form is Dove's polyphonic siren, which has been perfected by Helmholtz. Here is Seebeck's siren. I make it rotate, and open the little keys, and you hear the different tones. As I drive faster the notes rise up sharper and sharper. It gives, however, a very feeble and wretched note; the object in the latter instrument has been to produce a more powerful one. This double siren cannot be said to err on the side of lack of power. It has two perforated rotating discs on one axis, each connected with wind-chests, which are covered by outside boxes, so as to give more purity to the sound. The lower wind-chest is fixed, but the upper can be rotated upon its axis. Each of them contains four rows of oblique holes, and each of these four rows of holes can be brought into operation by touching a valve which draws in and out. These instruments require very great wind pressure; we have now a pressure on the bellows of more than one hundredweight; equal to a column of twenty-six inches of water. When this high-pressure wind is allowed to pass through the oblique holes in the wind-chest against the corresponding holes in the rotating disc, the latter is soon set into rapid motion. At first a series of puffs is heard, as the two sets of holes coincide and interfere, but these soon merge into a continuous hum, and mount into a note, which rises steadily in pitch as the rotation becomes more rapid. An attached counter enables the speed to be measured by means of a watch. If you examine the ratios as I have written them down on the black board, you will find that on the lower disc there is a row of eight holes, outside that a row of ten, then a row of twelve, and then a row of eighteen. These give us, if you take C for the lower note, E, G, and D. On the upper disc there are 9, 12, 15, and 16, which give D, G, B, and the upper C. We can thus get unison by taking the two G's; an octave by taking two C's, a 5th by taking C and G, and so we can work through the various consonant intervals. I have not time to demonstrate these facts fully, but I will consider one or two. Starting with unison on the two discs, I rotate the handle moving the top

box, whilst the lower one is sounding; you will hear that the sound rises and falls according as the phase of the vibration on the top disc is similar to the lower one and reinforcing it, or opposite to it and antagonizing it. In each rotation you thus get eight spaces, four of the loud, and four of the feeble sound. I can also show you the beats which arise from slightly altering the pitch of the upper tone by slowly rotating the upper box by means of the handle, the tone becoming flatter when the direction of revolution is the same as the disc, and sharper when it is opposite. You hear the beats very distinctly. These beats involve a most important question of acoustics which must be reserved to the next lecture. We thus reach the second part of the subject, namely, reinforcement and distribution.

Now reinforcement is common to all kinds of vibrations, it even occurs in the child's swing. When a child swings itself it reinforces the action of the swing by slight muscular exertion. It occurs when peals of bells are ringing. Any one who has been at the top of Magdalen Tower at Oxford on the 1st of May, will have noticed that when the bells begin to ring the top of the tower rocks like a ship in a storm, people have even felt as if sea-sick. Although there is this pendulum motion, like that of a rod or tuning-fork, the tower is particularly safe, for it shows the foundation to be solid, the vibration occurring in the limestone which is elastic. The same reinforcement occurs when soldiers cross a suspension bridge. The rule is that when a regiment has to go over a light bridge they break step. If they march together the effect has been to injure or throw down the bridge. Reinforcement may be defined essentially as a gentle force, acting periodically, as when one pulls a heavy bell; each individual pull will not raise the mass or anything like it, even with your whole weight; but you keep on, reinforcing the vibrations of the bell, taking the period of the swing and giving each time a gentle impulse, and this gentle force acting periodically becomes magnified into a very great one. Generally speaking reinforcement in sound is correlative with the power of producing sound. All sounding bodies also reinforce, but some have been divided off by Clerk-Maxwell into what he terms distributors. Others again have the power of singling out particular sounds for reinforcement. You may hear this going on in many places. If you take up

one of these forks even while I am speaking to you, you can feel the particular notes that I speak of; it seems to vibrate when a certain note occurs. The same with a drum—while I was speaking I could feel distinct vibrations in the drum as each word came out of my mouth. The same with a piano—if you take off the dampers and speak into it, and then stop suddenly, you hear the piano singing out loudly with a sort of hum, the notes which you have been speaking or singing. Or even more simply than that. If any of you happen to be at a musical performance such as the opera, and put your finger on the top of the crown of your hat, you will find it acts like a membrane to reinforce particular sounds, giving a regular vibration; and then another note to which it

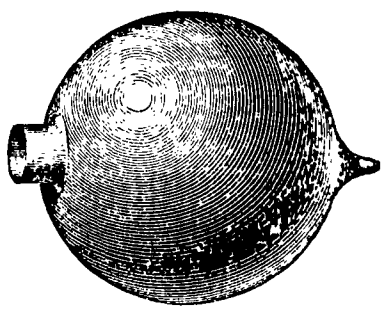


FIG. 8.—Helmholtz's Resonator.

does not consonate comes up, and the hat is still; so that the hat itself seems to be enjoying the music after its fashion. This propensity for singling out sound has been utilized by Helmholtz in making his resonators. He originally made the resonators, of which I have two sets here, with external membranes very much like this old marimba, but afterwards he found he could use the drum of the ear for the same purpose by making the cavity of a particular size, so that themselves speaking a certain note, they will single out that note from all others and reinforce it largely. Here is one which is sufficiently vibratile to give when struck a note like that of a bell, corresponding to the tone of E with 320 vibrations. When I speak that note you will hear it, and when I go down another note it ceases altogether. Here is another answering to G, and another to the upper C with 512 vibrations, and

so on. Here are others arranged for all sounds. Even as I am speaking, if you take a few and try them, you will hear as I fall on the particular note in the inflexions of speech the tube reinforces it. The human voice is very full of harmonics, and if I sing the low C different persons holding these resonators will hear the upper partial notes that I unconsciously produce at the same time as the grave note that I am consciously singing. In this way Helmholtz accomplished that wonderful feat of analysing the tones of different instruments and showing what musical quality depends upon, a fact which was never appreciated or understood before.

Now I have to speak of distributors. These are mentioned by Clerk-Maxwell as air, wood, and metal. The third form, the metal, has lately been revived in a very pretty toy which is being sold about the streets, and is called a telegraph. It is merely a couple of resonators made of pill-boxes or little boxes of tin, with a wire-thread passing between them. Any words spoken into one of these are perfectly audible at the other end. The first mode of distribution by means of air hardly needs much said about it, because it is happening at the present moment as I am speaking to you.

The second deserves a little illustration; the more so as it was studied by an illustrious man lately dead, Sir Charles Wheatstone. We have the original mechanism here by which he performed the experiment. This curious, classical-lyre-looking thing is the instrument he used in his form of the telephone. His distributors were simply bars of light deal. I have constructed a long bar of the kind with four pan-tile laths, along the side of the room, and I find even in this considerable length they convey the sound of a tuning-fork to this resonator very well. I will ask my assistant to strike the fork in the air and you will not hear it at all, but when he applies it to one end of the wooden rod, and I apply the other end to this resonator, you all hear it distinctly. The lyre down here is speaking the note produced at the top of the room. More than this, tactile sensation, as I have always contended, is shown to be continuous with aural sensation, for if I take hold of this rod while the tuning-fork is sounding, I can not only hear the vibration but I can feel it perfectly well at the same time; thus it is sensible to my auditory and tactile nerves transmitted through the length of wood. It is

very singular that this apparatus should not only reproduce pitch but also quality. When it was tried many years ago at the Polytechnic, some of you may recollect a band of players was placed in a lower room, each playing his own instrument, and to each instrument was attached a long rod of deal. These rods, after passing through the floor, were fixed to harps in an upper room, and when the players played, one harp, standing before you by itself, seemed to play a violin, another a clarionet, another a double-bass, another a piano, the sounds being conveyed through the rods of wood and giving not only the pitch, but actually the musical quality of the generating instrument.

Lastly, I have to mention a perfectly new and very remarkable distributor of sound in the shape of electricity. It has long been known that a rod of iron when magnetized by a galvanic current gives a peculiar clink; that I propose to show you first.

I have put the clinking apparatus in the middle of the room. It is a rod of iron surrounded by a long coil, and here I have the means of passing a current through the coil; the current first passes through a harmonium reed, a wire attached to the vibrating end of which dips into a mercury cup, so that rapid vibrations can be produced. I can transmit the vibrations of the reed up into the coil and thus intermittently magnetize the bar of iron. Those who are near it can hear the clinking no doubt, though it is faint. That clinking rises to a musical sound when the intermissions become more rapid.

Some years ago Reuss utilized this. His plan was to have a small box, such as I have here, with the vibrating membrane at the top connected with a battery. If you speak into the box and the membrane vibrates, making an intermittent contact, the vibration is reproduced at the other end of the circuit, as you will hear when I sing into the instrument. The tone is now reciprocated by the receiving instrument at the other end. That is an early stage of the telephone. I am proud of being able to show (through the kindness of Mr. Latimer Clark) the new instrument invented by Mr. Elisha Gray, of Chicago, which has come over for this Exhibition. There are four springs, vibrating like tuning-forks, which are kept in motion by electricity. They give the common chord. Then there is an arrangement by which the vibrations that each of them

sends off can be transmitted through the line of wire to the distant receiving instrument. There are the four notes of the chord, and you can distinctly hear the pitch of each particular note sent from one end by means of a key, repeated at the other end of the wire. This is a very remarkable transformation of energy. What we have done before is comparatively easy to understand. We have sent the actual vibrations of a sounding body through thin pieces of wood; but in this instrument we have transformed vibrational energy into another form of molecular force, electricity, one which we consider to be probably vibratory, though the point is still *sub judice;* then we transmit the force along a metal wire miles away, and at the further end we are able to re-analyse it back into sound vibrations once more. It is likely to prove of very great value practically. For instance, certain receiving instruments will only respond to their own forks, and in telegraphing you will easily understand how it would keep perfect secrecy. In military service it would be possible to have a telegraph set up like this, so that you, carrying the right receiving instrument, would not be liable to what was often done in the American War, namely, to have a wire "tapped" and the messages carried off by the enemy.

ON TEMPERAMENT.

LECTURE II.

To-day I have, at the request of the authorities, undertaken to give a brief analysis of a more difficult and more theoretical but certainly a not less important subject. You must excuse me if it is a little dry. Yesterday we had abundance of experimental assistance; to-day much of our time must be occupied in even giving an outline of the subject, with figures and diagrams.

The subject is musical instruments and temperament, or rather, temperament as applied to musical instruments; and here at the very beginning I must notice that there has been much difference of opinion on the question. I find statements as opposite as these. Col. Perronet Thompson, of whom I shall have to speak further on, in his work on *Just Intonation* says, "The temptation under the old systematic teaching to play out of tune, was that performers might play with perfect freedom in all keys, by playing in none; hence the rivalry in the magnitude of organs, and sleight of hand and foot to conceal; but a reaction is setting in, and the world is finding out that music is not in noise, but in the concord of sweet sounds." On the other hand Dr. Stainer, a most competent musician and theorist, who has published an excellent work on harmony, writes in this way, "When musical mathematicians shall have agreed amongst themselves on the exact number of the divisions necessary in the octave; when mechanicians shall have constructed instruments on which the new scale can be played; when mathematical musicians shall have framed a new notation which shall point out to the performer the ratio of the note that he is to sound to the generator; when genius shall have used all this new material to the glory of art, then it will be time enough to found a new theory of harmony on a mathematical basis." Now I have once before demurred very strongly to this mode of

treating the subject, and since my original remarks at the conference I have had a little conversation with Dr. Stainer. Dr. Stainer, who, I need not say, is not only a most able, but a most unbiassed judge in the matter, quite admits that he does not hold those views so strongly as he did; that he is beginning to think there is a possibility of just intonation, although he sees very clearly the mechanical difficulties which we all admit, and which stand in the way of its production.

Going into detail as to what temperament is, we may define the object of it as being the division of the octave into a number of intervals, such that the notes which separate them shall be suitable in number and arrangement for the purposes of practical harmony. This will be probably new to many persons. The old form of harmonium, piano, and every keyed instrument, is so engraven on our minds from use, that most persons are quite unaware that there is any other possible arrangement. They may perhaps in a museum have occasionally seen a strange-looking instrument, stranger even than the one I have here, but they have passed it by, under the impression that it was incomprehensible or worse. Now the usual instrument which we are accustomed to, has of course its own system of temperament, and that temperament although not the oldest is certainly the simplest, and is generally called the equal temperament. It divides the octave, as you see in the harmonium, into 12 equal parts, or semitones. If it so happened that the octave could be divided into 12 equal semitones, such that the other divisions, the 5th and the 3rd, should be in tune, it would be a very great boon, but unfortunately nature has not so ordained it, and the first point which I wish to insist upon is what probably has not been conceived by everybody, that the discrepancy, the difficulties, the errors which we have to get over, lie, not in our system of music, but in nature itself. Just as the diameter and the circumference of a circle are not commensurable to one another; so the 5th, the 3rd, and the octave are not commensurable. They do not come to actual agreement in an arithmetical way, and this is so very well given in Mr. Ellis's translation of Helmholtz's great work, that I will ask your leave to quote a few words. When speaking of temperament he says, "It is impossible to form octaves by just 5ths or just 3rds or of both combined, or to form just 3rds by just 5ths, because it is impossible by multiplying any one of the numbers $\frac{3}{2}$, or $\frac{5}{4}$ by two, or either by

itself or one by the other any number of times, to produce the same result as by multiplying any other of those numbers by itself any number of times." The ratios, you recollect, of the different notes of the octave to one another were briefly mentioned yesterday, and I have left the diagram up there; of course those ratios can be put in the form of fractions by putting the antecedent as numerator, and the consequent as denominator, so you easily form $\frac{3}{2}$, $\frac{5}{4}$, &c., as stated in Mr. Ellis's book. It is perhaps the simplest way of making you understand this incommensurability, to take a case. If we divide the octave into 12 equal semitones, of course the 5th ought to be seven of these; but it was found out very early in the history of music that the 5th is a little more than seven of these. As a matter of fact a 5th is 7·01955, and consequently, taking 12 of these 5ths, they give rather more than 7 octaves. They do not come back again to the corresponding octave of the note from which you started. This difference, or departure as it is termed, is the former figure multiplied by twelve. I will give you the multiplication by twelve for simplicity's sake. We have ·23460 of a semitone as the excess. This is an old discovery generally attributed to Pythagoras, and the figure is commonly called "the comma of Pythagoras." What a comma is, I shall presently show you. But it is questionable, as I mentioned before, whether Pythagoras deserves entirely the credit of this discovery, or whether he merely imported it from Egypt or Babylon. At any rate the Greeks knew, as I told you yesterday, of the monochord, the ratios to be derived from it, and of the divisions of the scale. Euclid wrote a work called the *Sectio Canonis*, or the division of the string, which contains all these given in very full detail. The 3rd of their scale was made in a similar way by four 5ths taken upwards, and that is still called a Pythagorean 3rd. There is then an incommensurability between the octave and the 5th which is in nature, and this incommensurability when multiplied gives the interval we term the comma. The error of the Pythagorean 5th has been said, and is still said by some persons, to be too trifling to be noticeable; that human ears are unable to appreciate it, and that you can overlook it. I believe I can show you distinctly the opposite in two ways. I can show it on this harmonium where I can play two notes differing by a comma, or by a smaller interval which I shall have

to speak of presently, a schisma; and I am going to ask a friend of mine who has a fine tenor voice to sing a true interval, and then we will compare it with the tempered interval, as given by the common harmonium.

Here is Helmholtz's harmonium, on which I can show you very clearly what is a comma—it seems to me very audible, and perhaps Mr. Colin Brown will kindly play us a comma on his instrument, and a schisma also. The schisma is only $\frac{1}{11}$th of a comma, but I think you will agree with me it can be distinctly heard. Now if my friend Mr. Jones will sing a perfect 5th we will have the note sounded on the harmonium, and I think you will notice the difference distinctly. I admit the experiment is difficult, indeed only last night I heard it denied by a great authority that it could be shown. (*A true fifth above tenor G was sung, and the D of the tempered harmonium was shown to be distinctly flat to it.*) Now the question occurs, what is the best mode of getting over, of covering up, or in some way retrieving these inherent errors of the scale? Ever since the early times of harmonic music different plans have been suggested, some of them of considerable historical interest. The principal was what is termed the unequal temperament. It was used for organs in former times, and is now termed mean tone, or meso-tonic. English cathedral organs up to a recent period were tuned by this system, and traces of it can be found in the music written for them. Until lately I could have pointed out many organs, and I believe there are some still, tuned on this system. The organ of Canterbury Cathedral, with which I am somewhat connected, has only lately been shifted from the old unequal temperament, and the large organ in the Moorfields Roman Catholic Chapel was only a few years ago tuned on the unequal temperament. If you look carefully at the music of the time, Purcell's anthems, for instance, in Boyce's Cathedral music, you will be able to notice that he palpably shirks certain notes; he avoids G♯ for instance, because he knew G♯ to be a treacherous note on this old temperament. When the organ was altered at Canterbury a few years ago, some pipes had to be cut, and others had to be lengthened; the lengthening was very considerable, the larger pipes had to have no less than two feet of metal stuck on to them, so as to bring them into tune. What was this unequal temperament then? It was an attempt at getting the more common scales accu-

rate; scales in common use were tuned perfectly true, or very nearly true, and the error was accumulated in other keys which were supposed to be less needed; they were termed wolves. Of course, there was a consequent condition in dealing with this old tuning that the player should limit himself to a prescribed circle, and should never modulate into these forbidden keys; A♭ and B♮ were wolves; I speak from memory, as it does not matter which, so long as you understand that some were excluded. This temperament had many merits, and some organists even of the present time prefer it to the equitonic. I am not at all sure that I should not prefer it myself. It specially had the advantage of retaining the third of the scale correct; but it had on the other hand the fault of flattening the very sensitive fifth to its injury. Of this I shall speak again, but I may show you a table which I abbreviated from Mr. Curwen's work, who I believe derived it originally from Mr. Ellis, wherein I have put down the numbers respectively of the three temperaments. In the middle is the true temperament, there is the ordinary equal temperament on one side as used for pianofortes and harmoniums, and the unequal temperament on the other.

	Old.	Just.	Equal.
C	30103	30103	30103
B	27165	27300	27594
A	22320	22185	22577
G	17474	17609	17560
F	12629	12494	12545
E	9691	9691	10034
D	4846	5115	5017
C	0	0	0

The reason that this system became insufficient for the needs of players dates back as far as the time of Bach, who wrote a great work called *The Well-tempered, or Well-tuned Keyboard;* he therein aimed at getting perfect freedom of modulation into all keys; but the geniuses who first made use of this system and required it, were Mozart and Beethoven.

They were the first to find tempered instruments, which established the possibility of going round from key to key, wandering in beautiful modulations wherever they wished. This perhaps led them into the practice, and the desire for it became so strong that they sacrificed a certain amount of accuracy to obtain freedom of motion.

Before, however, we are in a position to compare the different systems of temperament, we ought to fix on a standard of comparison. You should understand that we are not dealing with ratios in speaking of this standard of comparison, but with absolute numbers; I gave you the ratios yesterday. A ratio may remain large, fixed, and simple, whilst the component numbers upon which that ratio is founded dwindle down by degrees to infinitesimal smallness and fractional complexity, or they may rise to equally large values at the other extreme. The ratio and the number are different things altogether. It is somewhat singular that in these days the mistake of confusing these two should be made. But it has been made, and is continually being made, therefore I feel bound to give you a warning against it. In dealing with absolute numbers we may employ two principal methods of estimating them. We may use the geometrical method and compare them as magnitudes, or the numerical, and compare them as numbers. The geometrical method can be shown very well in a large diagram kindly lent me by Mr. Ellis. This long column contains the four forms of temperament marked at the head of each column by their first letters. Here is the hemi-tonic, or equal semitone system; the just, the meso-tonic, or old organ tuning, and the Pythagorean systems follow. You will see that the length of the black, blue, red, and yellow, is made to designate each note, but as you go up and down the scale those lengths do not at all agree in the same place; one overtops another, and in another place falls short, thus exactly measuring the inherent error of the scale which we have somehow or other to get rid of. If they were all accurate to one another, we should not have that locking in of one with the other. That is the geometrical method of showing differences of temperament, but we may do it by means of numbers. That table admits of translation into numbers. I have one which I shall be happy to lend to any one who is interested to copy. By the numerical method there are a good many

different ways of indicating the equal semitone system. We may take decimals, but this involves long and unwieldy figures. Of course there is no reason why long and unwieldy figures should be unmanageable, but some people are afraid of them. I will write out in full that Pythagorean comma of which I gave you before the first few figures. It is 7·019550008654. Taking the third founded on that, it is 3·863137138649. That would be rather difficult to recollect, though we have worse numbers than this to deal with; for instance, the value of π which is not only larger, but goes on to all eternity and never stops. No doubt the first five figures of decimals would be sufficient for many purposes, but that is only an approximation. The question arises whether we cannot use smaller divisions than 12. Several methods have been adopted which are very convenient. 24 has been used, 31 is good, and also 50; 53 is remarkably good, and so is 118. I mean that instead of dividing the octave into twelve divisions, we may divide it into a larger number, and these are the several denominators or consequents of ratios which produce the best results. 53 is so good that I thought it worth while to make a diagram of it, and here is a scale of 53 divisions to the octave from *C* to *c*:—

5	*c*	octave	53
9	*b*	seventh	48
8	*a*	sixth	39
9	*g*	fifth	31
5	*f*	fourth	22
8	*e*	third	17
9	*d*	second	9
0	*c*		
53			

By adding these together you get the other intervals. For instance, if you take 9 and 8, and 5 and 9 together, they make 31, that is the 5th; or take 9 and 8 = 17, and that is the 3rd, the whole octave being divided into 53. This has another advantage, that you can show by it, in a very simple way, the comma; you will find that the comma comes out to be just one of these divisions, $\frac{1}{53}$rd of the octave. If you want to go to greater nicety, you must take a larger number of divisions, and of that I have also given some illustrations.

One of the best numbers to choose is $30103\overline{}$, which, in the decimal form, is really the logarithm of the number 2. We need not consider it as a logarithm, but simply treat it as a common number. If you separate the octave into this large number of very small divisions you can show all intervals with great accuracy, and even get the interval of a schisma. Taking the octave as 30103, the fifth will be 17609; the comma 539, and the schisma will be 49. Some put it at 48, but 49 has this advantage, that it is just $\frac{1}{11}$th of the comma. The table above is framed on the 30103 system. Now I am in a position to show you the comparison between the new and the old temperaments a little more closely. Here are three columns, one the just intonation, the second the old organ tuning, and the other the modern.[1] The old organ tuning had one advantage, that it keeps the 3rd perfectly correct, 9691, the same as in just intonation, but you see how terribly that is thrown out by the equal tuning being raised to 10034. On the other hand, the fifth is a little wrong; it is more out than in the equal temperament. The mode of working this out deserves a little consideration. You divide the octave into any number of intervals, of aliquot parts, which we may call m; v of these make a fifth, t represent the major 3rd, and q represent the comma; taking the logarithm of 2 which I have given in the form of a whole number, and dividing it by these, we get the closest possible cyclic approximation to just intonation. The cycle of 53, which has the advantage of simplicity, was first proposed by Mercator, who is known as the inventor of a plan for charts. This system was employed by Perronet Thompson, and has been fully carried out in that beautiful harmonium of Mr. Bosanquet's, which many of you may have seen in the galleries of the Exhibition on the other side. I did not think it desirable, as it is a bulky, heavy, and delicate instrument, to bring it over here, but you will probably have heard it played and explained by Mr. Bosanquet himself. For practical use there is no doubt that this 53 scale is the most perfect we can get without running on to a most impracticable number of divisions in the octave. Whether it is the best for performance upon a playing instrument is an entirely ulterior consideration of which I shall speak presently. This 53 scale gives you an opportunity of seeing

[1] See table on page 161.

how the comma of Pythagoras is got without unnecessary computation.

Twelve 5ths	$= 31 \times 12 =$	372
Seven octaves	$= 53 \times 7 =$	371
Therefore the comma	$=$	1

On the larger scale the comma rises to 539, and the schisma to 49 or $\frac{1}{11}$th of the comma.

Having now established our standard, we are in a position to compare the various systems; but before doing so, without intending disrespect to anybody, I must remark on the complex nomenclature with which we have here to deal. It is perfectly astounding. I do not propose to go into all of it, but I may simply mention it to show you what names exist. Amongst them I find, Commatic, Pythagorean or Quintal, Mean or Meso-tonic, Commato-Skhismatic, Hemitonic, Skhismatic, Skhismic, Skhistic, Cyclic, and Skhismo-cyclic. These words of course are of value for investigation, but there are too many, and they are too near to one another in sound for ordinary use.

The difficulty, of course, which all these systems were made to meet, is that the advance of music requires free power of modulation from every key into every other, both of the major and minor forms. We can obtain this in two ways; either by a slight falsifying of the intervals, or by increased mechanism and an increased number of notes in the octave. These two views are very well represented by the two quotations I gave you, from Perronet Thompson and from Dr. Stainer. Dr. Stainer at the time took, and in a great measure still takes the view, that the organ at present built is as complicated as it will bear being. Indeed, in speaking about it the other day he said that he should be very glad to adapt true temperament to the St. Paul's organ: but imagine the St. Paul's organ, which is now very large, with eighty-four keys in each octave! St. Paul's itself would not hold the organ, much less the congregation. There is great truth in that. Mr. Bosanquet's harmonium has eighty-four keys to each octave, and if you multiply that by the number of stops it would become so utterly unwieldy that practically no one could play upon it. In the equal temperament, as I have said before,

the octave is divided into twelve semitones, and the result of this is that the fifths are a schisma flat. This is not a great flattening, but the interval of the fifth is very sensitive. It very soon beats, as it is termed; that is to say, the interference of sounds caused by the flattening of the schisma produces about one beat a second. On the other hand, the equal temperament disfigures the third very much indeed. It makes it seven schismas too sharp. The sixth also, which is a very peculiar and beautiful interval, and which has been called the sorrowful sixth—(if I am not mistaken, the bagpipes derive their peculiar wailing effect from the use of the sixth which occurs in the archaic scale of that instrument)—that sixth is disfigured very much. The number is 22185 in just, and 22577 in equal temperament, or eight schismas too sharp. The seventh again, in the old temperament is rather flattened, as you see. The numbers are 27165 in the first column, and 27300 in the other; whilst it is terribly wrong in equal temperament, namely, 27594. There is another discrepancy in the tempered scale affecting the second. If I had time I could show you that this is a variable note, and requires to be used in two ways. In the old temperament it is about half way between the two, but in the equal temperament it is ninety-eight divisions too sharp for the acute form, and in the flat form it is nine schismas too sharp. This shows that the equal temperament is about as bad a system as we can employ. It has only one advantage, and that is that it is simple, and everybody can learn it easily. There is another accusation to be brought against it, though perhaps you may look upon this view of the question as rather Hibernian, namely, that we never get it; the tuning of the fifth a schisma flat, which gives one beat a second, is a delicate process, and I firmly believe that very few pianoforte tuners are really able to accomplish it. Mr. De Morgan used to say that he never could manage it, although he separately tuned a number of strings himself to beat one beat a second; for when he compared them together they never were in tune. Now if he could not do it on a single note with his great mathematical ability and mechanical skill, I doubt if the ordinary class of tuners can. However, although I object to equal temperament on these grounds, which you will see are obvious facts of nature, not at all matters of opinion, I must allow that it does afford great

facility and simplicity to have only twelve keys in the octave; it would be of great advantage if this facility could possibly be retained.

How then shall we go on instrumentally to improve matters? For the first method, I have here a form[1] of Helmholtz's harmonium which is really very little complicated. Any person understanding music can very soon master it. There are two keyboards put into the place of one, the lower of which is a comma sharper than the upper; consequently, when you want to lower any note a comma you can do it by putting your finger on the upper keyboard, instead of the lower one. This gets over a great many difficulties, but it is not absolutely true. However, the great fault of equal temperament is the third. If we have the third, sixth, and the seventh approximately true, we have got over the most important errors. Those intervals are to a great extent accurate on this instrument; the dissonance of the sharp third itself is not, I think, beyond the limits of audibility. Those who say so cannot possess very good ears. I will give you the common chord, first on the single keyboard, and then change to the third, a comma flattened, so that you may hear the difference. Now I will take the sixth. When you have heard the true interval you will see that the other is decidedly out of tune, although it might pass muster if you did not hear the correct interval. The first mode then is Helmholtz's double keyboard with 24 notes.

Then there is another contrivance equally simple in the keyboard; this is Mr. Ellis's harmonium. He accomplishes his object by shifting the sound not by means of separate notes or keyboards, but by combination stops. I wish I had the instrument here to show you; though if it had been here you would have seen nothing. It is just like a common harmonium, except that it has a few draw-stops which fulfil different functions from those in the ordinary arrangement.

The next is an instrument which you have probably all seen in the Exhibition—that of General Perronet Thompson. Perronet Thompson adopted the system of increasing the digitals up to the full number of sounds, though he did not carry it out quite to the bitter end; and therefore he does not profess that his enharmonic organ plays in all the keys; but even he has about 72 keys to each octave, and he starts on the

[1] Mons. Gueroult's, made by Debain.

cycle of 53 sounds, of which he uses about 40. With all that mass of keys of different kinds, described by differ-

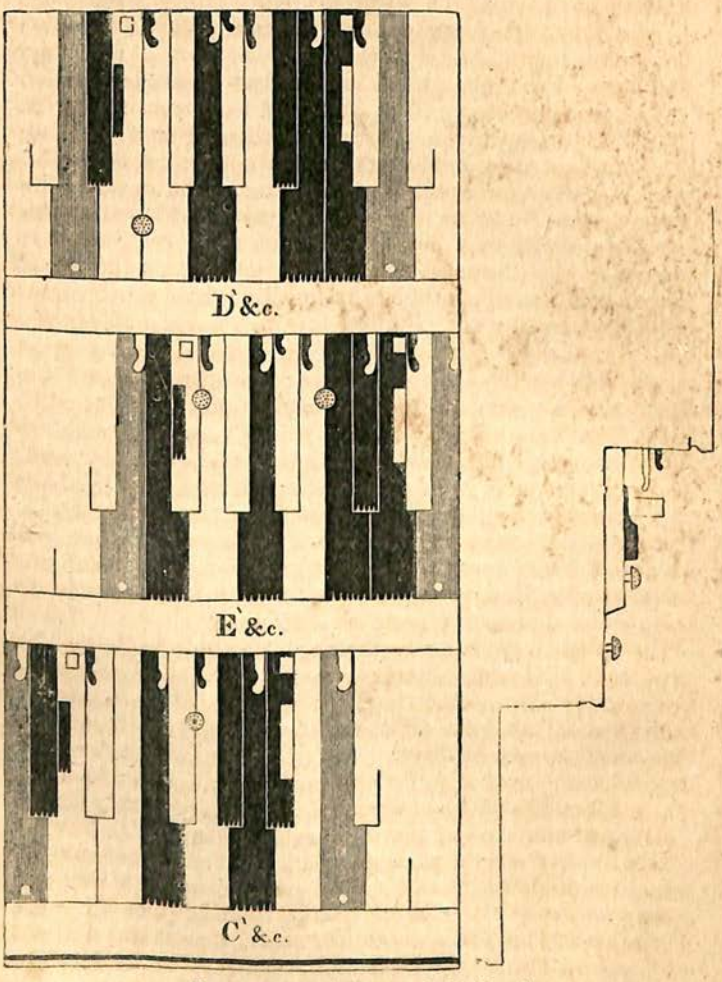


FIG. 9.—Perronet Thompson's Keyboard.

ent names, quarrils, buttons, and flutals, besides the usual twelve keys, it looks a very difficult instrument to play, and

has always been found unmanageable. It has the power, according to his own statement, of performing correctly in twenty-one keys with a minor to each.

If you look at the keyboard in passing, you will see three rows of ordinary keys, coloured in various ways. There are, in fact, three keyboards; there are also the quarrils, buttons, and flutals named above. With all the mass of mechanism which looks like a large organ, it only possesses one speaking stop. This seems a very small result for such enormous magnitude, and you can now appreciate what St. Paul's or any other large organ, with perhaps eighty speaking stops besides couplers, would be if it were magnified in the same ratio.

I have now to speak of two harmoniums, first that of Mr. Bosanquet, and secondly one, which I am happy to be able to show you to-day, of Mr. Colin Brown, Professor of Music in the Andersonian University of Glasgow, who has been kind enough to come all the way from Scotland to play it to you. In both these we seem more within bounds of practicability. Two things are aimed at in both these harmoniums which are not in those I first named; in the first place to get correct intonation, and in the second to generalize the keyboard. You do away with that distinction between black and white notes which causes so much confusion to learners. Mr. Bosanquet certainly has black and white notes still left after a fashion, so as to guide the eye, but you never play as you do on the common piano in six flats or five sharps and so on. You do not feel your way by the black and white notes as you have to do on the ordinary piano. There is only one key or scale on each; it is a little complicated, but when you have once learned it all positions are the same, and that is why Mr. Bosanquet terms his instrument a generalized keyboard. I have heard persons say, "I cannot play in five flats; I can play in one flat, five are too difficult." Here there is no such difficulty; five flats are no more difficult than one flat. You have only to get the right pitch and all scales are in the same position for the hand. I think I have stated that Mr. Bosanquet has fifty-three sounds in the octave, though there are eighty-four keys. This harmonium possesses also some very beautiful characteristics, of which one is the power of getting the harmonic seventh. Nevertheless he does not look upon it as an instrument for great execution. It is more intended as a sort of well-spring and fountain from which can be drawn

pure chords, vitiated and made dissonant by more ordinary instruments. I admit that my own sense of hearing was incorrect at first. I did not like these purely consonant untempered instruments. When I first heard Perronet Thompson's organ, I thought the intervals were what tuners would call "too keen." I was accustomed to the universal dulling, dumbing of the scale which one hears from an equally tempered instrument, all the notes being thrown a little out of tune; but if you go and sit beside this harmonium, after a little while, when the first effect of novelty is worn off, you will come to like it very much indeed. Our senses are more or less injured by long practice with the other system, and therefore Mr. Bosanquet, I believe, wishes it to be employed by composers to get combinations, to see what they can use in proper intonation, and afterwards arrange for instrumental performance of them by other means. It is intended more to manufacture music upon than for performance.[1]

Mr. Colin Brown is here himself, and he will correct me if he wishes, but I hope he will not disagree with what I say, that he aims at a slightly different object; namely, at getting just intonation in the simplest fashion, and with the least complicated keyboard by which it can be obtained.[2] His keyboard is by no means so elaborate as Mr. Bosanquet's, and is therefore more suitable for accompanying purposes. This it seems to me is a very excellent direction to take. We must make a compromise. Absolute truth is not to be had here; we shall never be able to obtain elaborate execution on instruments like Mr. Bosanquet's or Perronet Thompson's, but here is a harmonium which I believe can be learned in a short time, and which is in some respects even easier to learn than the ordinary keyboard; yet you can produce upon it just intonation to a very considerable if not to the last possible degree. Mr. Brown has in this particular harmonium twenty-nine sounds to the octave; some which are now making (for this is only an experimental instrument, intended to try the arrangement), but which have been delayed by the illness of the workman, will have thirty-two sounds to the octave, and the whole scale can be completed if

[1] For a more detailed account of this instrument, with a diagram of the keyboard, see Appendix I., kindly contributed by Mr. Bosanquet himself.

[2] See Appendix II.

necessary by putting in forty-four sounds to the octave. I shall ask you to listen to it presently, but I am anxious to conclude first my own task as to the application of true temperament to other than keyboard-instruments.

I have spoken hitherto entirely about organs, and harmoniums. With organs not much has been done; whilst harmoniums have occupied most inventors, because they are instruments which show dissonance more than any others owing to the peculiar quality of tone they give out. They are liable to painful interference and harshness of tone. For these reasons they are not liked by many persons. They are, however, very convenient instruments, not at all expensive, nor liable to get out of tune; therefore they are seen in many places where you do not find a piano.

If we can get this true intonation by a moderate amount of mechanism, and at a moderate price, we shall have a harmonium which will play as sweetly as an organ or a piano. For the piano true intonation does not appear to be so necessary, because it has only an evanescent sound, the note being produced by a blow. It hardly causes continuous beats; at any rate they are not so audible; indeed the ear requires to be practised, to have learned the unpleasant art of detecting the beats; when you have once acquired it, you become terribly sensitive to ordinary music; for with the equal temperament, and with the errors which I have pointed out in it, we never get an instrument perfectly in tune.

Now for the application of this method to orchestral instruments. We have made a beginning. There is in the Exhibition a trumpet invented by a friend of mine in which the valves are three, but the third valve, instead of altering the pitch by a tone and a half, as it usually does, alters it by a comma; therefore Mr. Bassett calls it the "Comma trumpet." Whether that particular instrument is or is not successful, I need not here mention, but the idea carried further may be fruitful in good results, because if you can alter any dissonant note a comma up or down, you can produce much more perfect harmony in the orchestra. I am doing the same thing with the clarionet and oboe. Here is a clarionet, only an ordinary one, as I am anxious not to complicate the mechanism more than necessary; but by means of double keys it will produce a great many accurrate intervals. For instance, I can take the E flat in two forms, and the F in three different

ways. Several other notes have alternative fingerings, so that I hope to manufacture reed instruments with which we can get, in many keys if not in all, true intonation. We do not require it in all keys with the clarionet, because as you are aware, players use different instruments for different keys. There is the B flat clarionet, which is useful for the flat keys; to play in sharp keys there is the A; and there is also a C clarionet, though it is less used. In this way, by having just intonation for one or two keys on either side of the natural key, I believe we shall arrive at more perfect results. It is very desirable it should be so, and I hope we shall be able to carry it out. I will here conclude the talking part of the lecture, but the most important part you will all agree with me will be the description of his new harmonium which Mr. Colin Brown has so kindly undertaken to give us. Before playing he wishes to explain the system on which the keyboard is arranged.

Mr. BROWN.—The construction of this instrument arose from a series of experiments in analysing a musical sound. It is mathematically and musically correct, and contains neither compromise nor approximation of any kind from beginning to end of the fingerboard. The octave consists of seven digitals, with one added for the minor scale. It involves no complicated calculations, for there are only seven musical relations or differences on the keyboard, and these require neither decimals, nor logarithms nor equations, to express them.

The scales run horizontally along the instrument, the keys across it, scales and keys being at right angles. The progression of fingering the scale in all keys is the same, and as no extra digitals, such as the five black upon the common keyboard, are required to play chromatic tones, this fingerboard is called the "natural fingerboard."[1]

Instead of beginning where, as Dr. Stone pointed out, is usual, with the larger intervals of the scale, as the octave and fifth, I have begun at the other end of the scale with the first elements—8 : 9—9 : 10—15 : 16.

Though the larger intervals of the scale are relatively incommensurable, by starting with these primary relations we find that every interval is accurately produced from them;

[1] See Appendix II., kindly contributed by Mr. Colin Brown.

thus $\frac{9}{8}$ added to $\frac{10}{9}$ give $\frac{5}{4}$, the major third; and these, added to $\frac{16}{15}$, give $\frac{4}{3}$ the perfect fourth; and so on—$\frac{9}{8}$, $\frac{10}{9}$, $\frac{16}{15}$, $\frac{9}{8}$, $\frac{10}{9}$, $\frac{9}{8}$, $\frac{16}{15}$ added together give $\frac{2}{1}$ or the octave.

In addition to these three relations, $\frac{9}{8}$ less $\frac{16}{15}$ gives 128 : 135, or the chromatic semitone; thus there are in the scale two tones, $\frac{9}{8}$ large and $\frac{10}{9}$ less, and two semitones, $\frac{16}{15}$ diatonic and $\frac{135}{128}$ chromatic.

Besides these four relations, the three musical differences of the scale are also to be found on this instrument, viz., $\frac{10}{9}$ less $\frac{16}{15} = 24 : 25$, the imperfect chromatic semitone—$\frac{9}{8}$ less $\frac{10}{9} = 80 : 81$, the comma—and the schisma 32,768 : 32,805, which is also deduced from these relations.

The comma is the difference between the large and less tones or steps of the scale.

The schisma is the difference between a sharp tone and a flat one, say between D♯ and E♭.

The round added digital in each octave does not belong to the series of the major scale; it produces the major seventh and sixth in the minor scale, and also introduces the imperfect chromatic semitone of 24 : 25, being $\frac{10}{9}$ less $\frac{16}{15}$. The effect of this additional digital is very peculiar; the tone seems to be too flat till it is heard in the chord.

These seven are all the primary musical relations and differences to be found on the natural fingerboard; there are many secondary keys to which I have paid no attention, —they may be very beautiful, but I do not enter into that question—for I have yet to learn, first, if they are true, and, secondly, if they are necessary or useful. The introduction of a round digital, placed on the white as well as on the coloured digitals, would supply every secondary key that the most exacting musician can demand; but I was anxious to avoid anything that would complicate or confuse the fingerboard.

Musicians have greatly to complain of mathematicians for carrying their formulæ beyond their legitimate sphere into the domain of music; and mathematicians, on the other hand, have to complain of musicians for demanding a number of secondary keys which are not mathematically or musically in true key relationship. It would be quite easy to make an instrument with them all, and at no great cost of money; but what would be the advantage?

In constructing this instrument the principles I had to consider were: a mathematical series of sounds on the one

hand and a musical series on the other; I had to reconcile and adjust their differences, and mark their coincidences. These coincidences are represented by the digitals on my fingerboard. I began with the simplest elements of the scale; and had to feel for and find my way at every step. This instrument is limited in its range, embracing only eight major keys with relative minors. It has only one keyboard of three levels, the addition of another level would complete the cycle of thirteen keys; it may, however, be extended to any range, C being always the central key—from C♭ grave to C♯ acute are given in the plan lodged in the Patent Office.

The question is often asked, How can such an instrument be played upon? No musical instrument can be of any real practical value unless it can be easily played. I shall not offer my own opinion upon this point, but that of a gentleman who has studied the subject thoroughly and whose opinion may be relied upon. After examining the fingerboard carefully he remarked,—first, that any person understanding the relation of keys in music will comprehend the principle of this keyboard in a few minutes; secondly, that any person who can play upon an ordinary harmonium will play just as well upon this fingerboard by two or three weeks' practice.

These two statements have been already amply verified, for every one who has tried to play upon this instrument half-a-dozen times has done so readily. The third statement made by my friend was that any person learning to play will save from two to three years usually spent in practising keys, because the scale in every key is played upon the natural fingerboard by the same progression of fingering.

Some time must elapse before this last can be verified—but there can be little question as to its correctness.

APPENDIX I.

BOSANQUET'S GENERALISED KEYBOARD.

In the enharmonic harmonium exhibited at the Loan Collection of Scientific Instruments, South Kensington, 1876, there is a keyboard which can be employed with all systems of tuning reducible to successions of uniform fifths; from this property it has been called the "generalized" keyboard. It will be convenient to consider it first with reference to perfect fifths; it is actually applied in the instrument in question to the division of the octave into fifty-three equal intervals, the fifths of which system differ from perfect fifths by less than the thousandth part of an equal temperament semitone.

It will be remembered that the equal temperament semitone is the twelfth part of an octave. In the present notice the letters E. T. are used as an abbreviation for the words "equal temperament."

The arrangement of the keyboard is based upon E. T. positions taken from left to right, and deviations or departures from those positions taken up and down. Thus the notes nearly on any level are near in pitch to the notes of an E. T. series; notes higher up are higher in pitch; notes lower down lower in pitch.

The octave is divided from left to right into the twelve E. T. divisions, in the same way, and with the same colours, as if the broad fronts of the keys of an ordinary keyboard were removed, and the backs left.

The deviations from the same level follow the series of fifths in their steps of increase. Thus G is placed $\frac{1}{4}$ of an inch further back, and $\frac{1}{12}$th of an inch higher than C; D twice as much; A three times, and so on, till we come to C′, the note to which we return after twelve fifths up; this note is placed three inches further back, and one inch higher than the C from which we started.

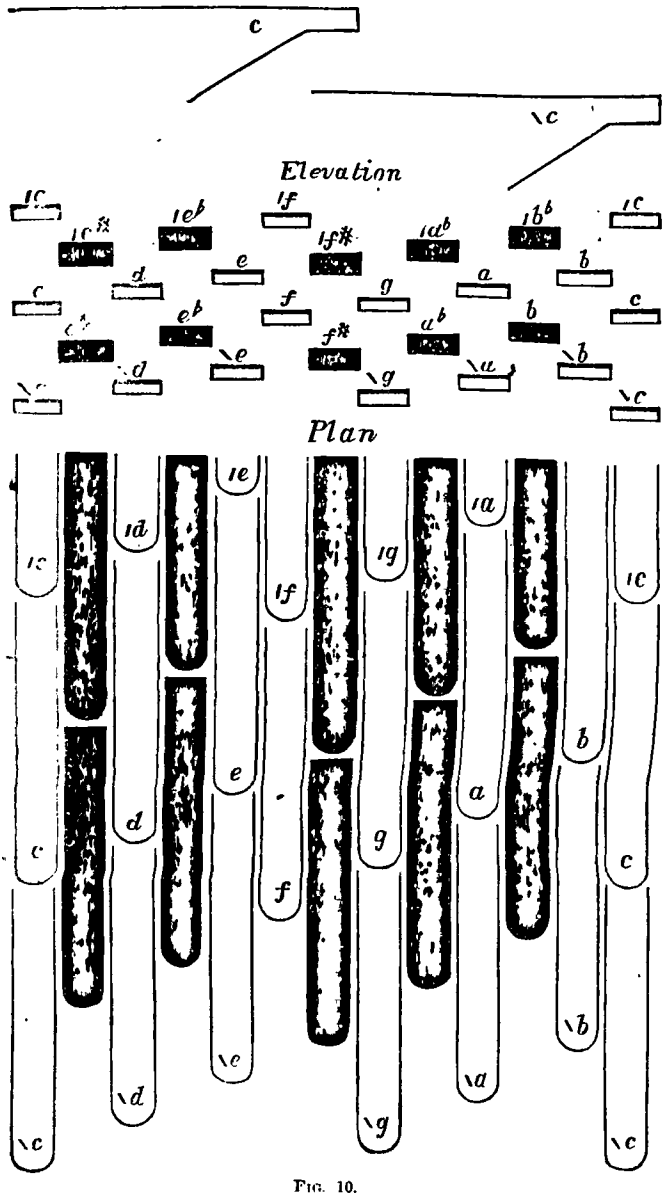


Fig. 10.

With the system of perfect fifths the interval, C—C′, is a Pythagorean comma.

With the same system, the third determined by two notes eight steps apart in the series of fifths (C—C′), is an approximately perfect third.

With the system of fifty-three the state of things is very nearly the same as with the system of perfect fifths.

The principal practical simplification which exists in this keyboard arises from its arrangement being strictly according to intervals. From this it follows that the position relation of any two notes forming a given interval is always exactly the same; it does not matter what the key-relationship is, or what the names of the notes are. Consequently, a chord of given arrangement has always the same form under the finger; and as particular cases, scale-passages as well as chords have the same form to the hand in whatever key they are played. A simplification which gives the beginner one thing to learn, whereas there are twelve on the ordinary keyboard.

The keyboard has been explained above with reference to the system of perfect fifths and allied systems; but there is another class of systems to which it has special applicability —the mean-tone and its kindred systems. In these the third, made by tuning four fifths up, is perfect, or approximately perfect. The mean-tone system is the old unequal temperament. The defects of that arrangement are got rid of by the new keyboard, and the fingering is remarkably easy. The unmarked naturals in the diagram present the scale of C when the mean-tone system is placed on the keys.[1]

[1] For further details on this important subject readers are referred to the forthcoming work *An Elementary Treatise on Musical Intervals and Temperament*, with an account of an enharmonic harmonium exhibited in the Loan Collection of Scientific Instruments, South Kensington, 1876; also of an enharmonic organ exhibited to the Musical Association of London, May, 1875, by R. H. M. Bosanquet, Fellow of St. John's College, Oxford. London: Macmillan and Co., 1876.

APPENDIX II.

THE NATURAL FINGERBOARD WITH PERFECT INTONATION.

THE digitals consist of three separate sets, of which those belonging to four related keys, representing the notes 2, 5, 1, 4, are white; those belonging to three related keys, and representing 7, 3, 6, are coloured—the small round digitals represent 7 *minor*, or the major seventh of the minor scale. These are the same in all keys.

This fingerboard can be made to consist of any number of keys.

The scales run in the usual order in direct line, horizontally, from left to right *along* the fingerboard.

The keys are at right angles to the scales, and run vertically *across* the keyboard, from C♭ in the front to C♯ at the back, C being the central key.

The scale to be played is always found in direct line, horizontally between the key-notes marked on the fingerboard, but the digitals may be touched at any point.

The order of succession is always the same, and consequently the progression of fingering the scale is identical in every key.

The 1st, 2nd, 4th, and 5th tones of the scale are played by the white digitals; the 3rd, 6th, and 7th, by the coloured.

The sharpened 6th and 7th of the modern minor scale are played by the round digitals. The round digital, two removes to the left as in the key of B flat, is related to that in the key of C as 8 : 9, and supplies the sharpened sixth in the relative minor of C; so in all keys similarly related.

Playing the scale in each key the following relations appear—

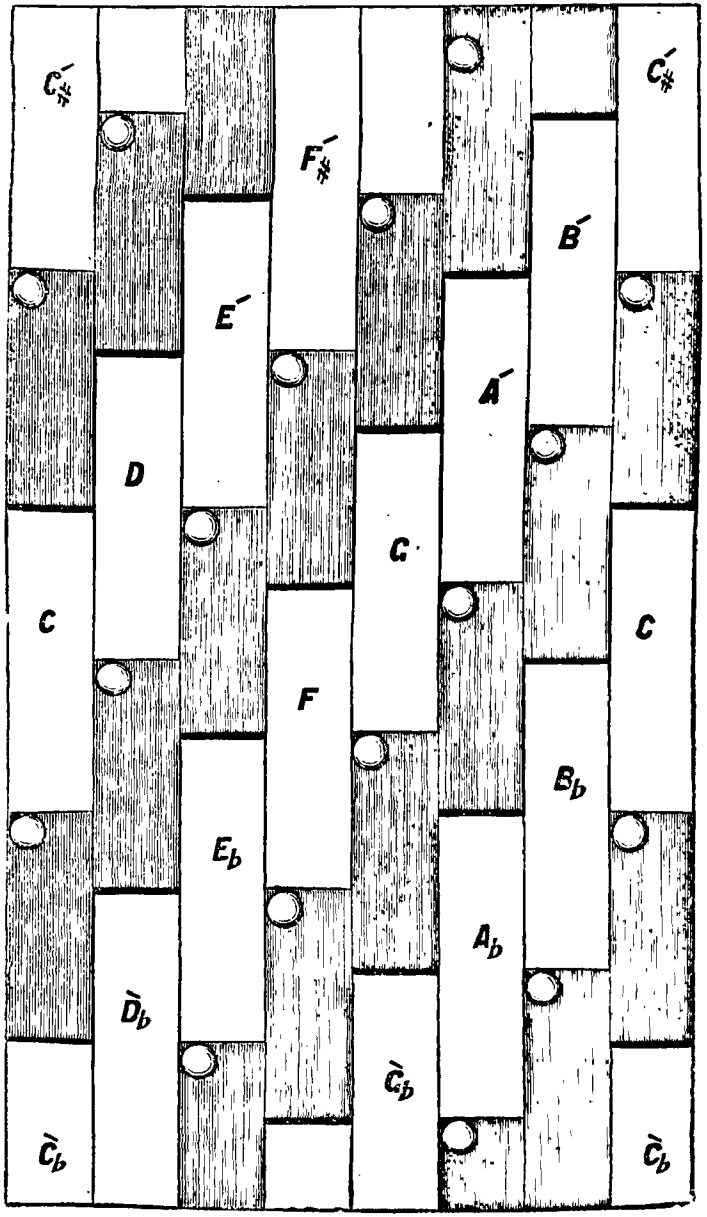


Fig. 11.—Plan of Natural Fingerboard.

From white digital to white, say from the 1st to 2nd, and 4th to 5th of the scale, and from coloured to coloured, or from the 6th to the 7th of the scale, the relation is always . . . . . 8 : 9

From white to coloured, being from the 2nd to the 3rd, and from the 5th to the 6th of the scale . . . . . 9 : 10

From coloured to white, being from the 3rd to the 4th, and from the 7th to the 8th of the scale . . . . . 15 : 16

From *white* to *white*, or *coloured* to *coloured*, is always the large step.

From *white* to *coloured* is always the less step.

From *coloured* to *white*, the diatonic semitone, or the small step.

The round digital is related to the coloured which succeeds it as 15 : 16, and to the white which precedes it as 25 : 24, being the imperfect chromatic semitone.

Looking *across* the fingerboard at the digitals *endwise*, from the end of each white digital to the end of each coloured immediately above it, in direct line, the relation is always 128 : 135, or the chromatic semitone; and from the end of each coloured digital to the white immediately above it, in direct line, the comma is found 80 : 81.

Between all enharmonic changes, such as between A♭ $404\frac{4}{81}$ to G♯ 405, the interval of the schisma always occurs, 32,768 : 32,805, the difference being 37.

These simple intervals and differences, 8 : 9—9 : 10—15 : 16—24 : 25—80 : 81—128 : 135—and 32,768 : 32,805, comprise all the mathematical and musical relations of the scale. The larger intervals of the scale are composed of so many of 8 : 9—9 : 10—and 15 : 16—added together.

The digitals rise to higher levels at each end, differing by chroma and comma, or comma and chroma alternately; this causes separate levels on the fingerboard at each change of colour; though these are not essential, they will be found very useful in manipulation, and serve readily to distinguish the different keys.

The two long digitals in each key are touched with great convenience by the thumb. The lower end of each coloured digital always represents the 7th in its own key, and

the borrowed, or chromatic sharp tone, in every other, thus, the 7th in the key of G is the sharpened fourth, or F sharp in the key of C,—and so in relation to every other chromatic sharp tone.

The white digital is to every coloured digital as its chromatic flat tone, thus, the fourth in the key of F is B ♭, or the flat seventh in the key of C,—so in relation to every other chromatic flat tone. In this way all chromatic sharp and flat tones are perfectly and conveniently supplied without encumbering the fingerboard with any extra digitals, such as the black digitals on the ordinary keyboard, the scale in each key borrowing from those related to it every possible chromatic tone in its own place, and in perfect intonation.

The tuning is remarkably easy, and as simple as it is perfect.

While all the major keys upon the fingerboard, according to its range, have relative minors, the following, 'B ♭, 'F, 'C, 'G, 'D, A, E, B, F ♯, C ♯, G ♯, and D ♯, can all be played both as major and as perfect tonic minors.

These secondary keys are more than appears at the first inspection of the fingerboard. A series of round digitals placed upon the white, and a comma higher, additional to those placed upon the coloured digitals, would supply the scale in every form the most exacting musician could desire, but it is a question if such extreme extensions are either necessary or in true key-relationship—and whether simplicity in the fingerboard is not more to be desired than any multiplication of keys which involve complexity and confusion.

NOTE.—A full description of the voice harmonium may be found in the specification for patent. The principles upon which it is constructed and tuned will be found fully stated in *Music in Common Things*, Parts I. and II., published by Messrs. William Collins, Sons, and Co., Glasgow, and Bridewell Place, New Bridge Street, London, and the Tonic Sol Fa Agency, 8 Warwick Lane, London, E.C.

APPENDIX III.

SINCE the delivery of the above lectures, another harmonium has been sent to the Loan Collection by Herr Appunn, of Hanover, on the system of true temperament. The detailed account of its mechanical appliances has not as yet arrived; but it may be briefly described as having thirty-six keys playing thirty-five perfect fifths; the major thirds being tuned by eight fifths downwards, that is, a *schisma* too flat. These are arranged in one row of keys, with two rows of buttons or studs. The keyboard is double, with an extra row of twenty-four tones arranged on the ratio 16 : 19 with respect to the two bottom rows, so as to compare the effect of the minor chord, using 16 : 19 with the usual 5 : 6.

SENSITIVE FLAMES AS ILLUSTRATIVE OF SYMPATHETIC VIBRATION.

BY PROFESSOR BARRETT.

THE subject assigned to me for the present lecture is Sensitive Flames. I do not propose, however, to deal with this subject exclusively, but rather to make it the goal which we shall gradually approach. By this means I think you will find that so far from being a strange and isolated phenomenon, a sensitive flame is really a striking illustration of a very widespread and important law relating to the reception of vibratory energy.

It will be instructive for us if we first regard the mode of production of the energy of vibration, then how this energy is communicated from one system of bodies to another. The simplest mode of exciting vibratory motion is that seen in the swinging of an ordinary pendulum. The to-and-fro motion which you see there gives rise to waves in the air of the simplest kind. In the case of the pendulum we have the motion sustained by the action of gravity; but in the case of a tuning fork we have the motion sustained by the elasticity of the metal. The nature of the vibration is, however, the same in both cases. The motion of the prongs of the fork to and fro is precisely analogous to the motion of a pendulum swinging to and fro. These are instances of the simplest form of vibratory motion, namely, the vibration of a body sensibly as a whole.

These vibratory motions generate simple wave-forms. Such a motion may have a uniform rise and fall across the line of rest. But there are other less simple modes of

vibration by the body as a whole. For example, the kind of motion due to the clapper of a clock bell, or that of a tilt hammer. First there is a slow uniform rise, then a sudden rapid fall. A violin string, bowed near one end, gives a similar mode of vibration.

Now, a body may vibrate not only in that simple form, but in a more complex manner. In fact, simple motions and simple waves are extremely rare. In general, compound tones and intricate wave forms are produced by the subdivisions of the vibrating body; for a body may split up into sensible parts, and these vibrate. Here, for example, in this monochord we may have the wire vibrating as a whole, or we may have it vibrating in a certain number of aliquot parts. This also is seen most strikingly in the vibrations of a plate. Here is a round plate of metal which, in its simplest form of vibration, divides into four equal parts; but it may be made to subdivide itself into a greater number of parts by fingering the edge of the plate. Thus if I draw a fiddle-bow over the edge of the plate, we get, you see, a division into a certain number of vibrating parts which are seen by the motion of the sand towards the nodal lines, or lines of rest. Now, such subdivision gives rise to intricate wave forms, and confers upon a body this peculiar quality of tone or twang which enables us to distinguish the notes of different musical instruments from one another, although they may be sounding notes of exactly similar pitch.

So far, then, we have seen the vibration of a body giving rise to sound-pulses, or motions of the air around it. But a body may vibrate also in its *insensible* parts, and such vibrations give rise to the phenomena of light, heat, and possibly of electrification Here, too, in molecular, as in molar motion, we may have simple and complex vibrations. For example, black hot elementary gaseous bodies yield, as one would expect, simpler modes of vibration than those corresponding to compound bodies; and in the case of solids and liquids an increasing temperature gradually liberates these substances from the mutual cohesion of their parts, and hence, ultimately enables them to vibrate without hindrance in the definite rates peculiar to each element. The body is now a glowing gas, and if in this state it be examined by the prism, it is found that at an

extremely exalted temperature, a few of the elements yield simple vibrations corresponding to the vibrations of a pendulum or tuning-fork. For example, the metal platinum and the metal sodium approach these simpler or pendular vibrations, having spectra of extreme simplicity. Other elements yield a complex vibration corresponding to a compound tone, giving rise, therefore, to intricate wave forms. We may instance, as examples of this quality, iron and chromium, and as there is a definite relationship between the upper partial tones of a compound tone and its fundamental tone, so the question has been asked, "Is there any definite relationship existing between the various constituent vibrations of a glowing molecule of iron and a glowing molecule of chromium?" This question has been answered, in fact, with some success by Mr. Johnstone Stoney. Time will not allow me to enter more fully into this matter, but it seems to open up a pathway for future discoveries.

Let us now, for a moment, regard the transference of this vibratory energy to our senses, or our instruments. It is evident that some medium is necessary. The coarser molar vibrations of sound are readily and rapidly transmitted by solid, liquid, and gaseous bodies. For example, if I take this long rod and hold it against a wooden surface, we get here transmission of the vibration of a tuning-fork through the rod to the wood in the distance. So, in like manner, water is found to transmit the vibrations of sound, and air is found to do the same, as in the case of the sounds which I am now uttering. The rate of the transference of this molar vibration to the medium around has been examined with great care by physicists both theoretically and experimentally. It will be needless for me to refer to the investigations now, but this piece of apparatus which is exhibited in the Loan Collection is extremely interesting, as being the very instrument whereby the velocity of sound in water was determined by those eminent physicists Colladon and Sturm (Fig. 1). They plunged this large spoon-shaped metal tube into the water, and at some distance off a boat was moored, and under the water near the boat a bell was struck. The moment that the bell was struck, a brilliant light was made to appear in the boat. The observers in a distant boat noticed the

interval of time between the production of the light and the reception of the sound through the water by means of this apparatus. Inasmuch as light passed over that small interval in an infinitesimally small space of

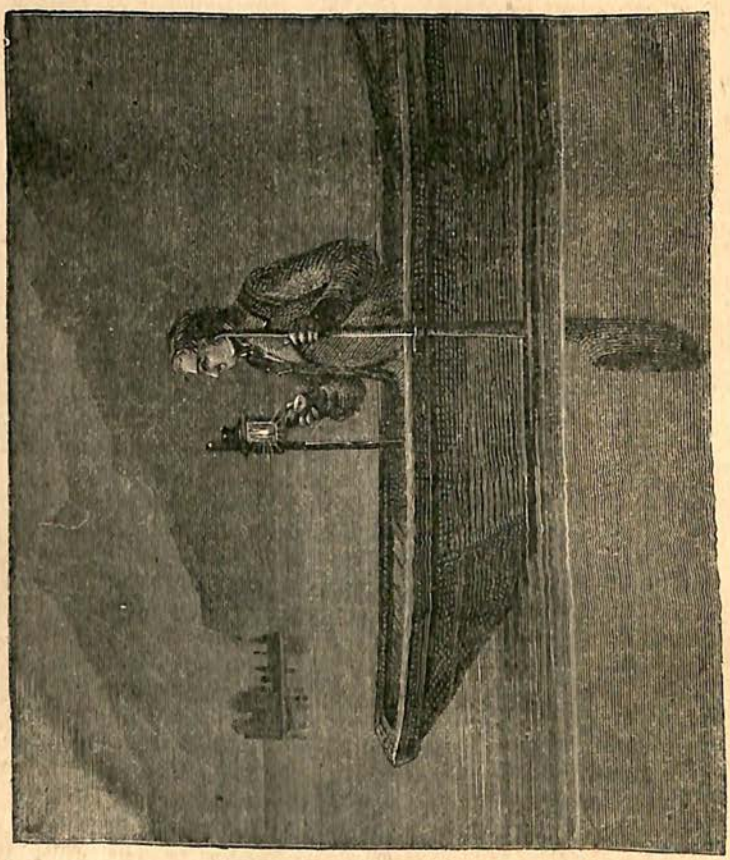


FIG. 1.

time, the difference between the perception of the light and the perception of the sound would give us the velocity of sound through that small space of water.

Molecular vibrations are, however, unable to be transmitted by the coarser matter around us. A medium of higher elasticity and of less density is necessary for the transmission of the vibrations of heat and light. Such a medium has been assumed to exist, and we have every reason to believe that it does exist; it is termed "the ether." As you have had, in preceding lectures by Professor Forbes, some fuller reference to this subject, I need not dwell upon it here; and therefore we will pass at once to the manner in which vibratory energies are accepted. We have seen how they are produced; we have seen that they are transmitted; and now we have to examine how they are accepted.

And here we meet at once with a very important law. This law is stated as follows:—The receptivity of a body for vibratory energy depends on the capability of that body to vibrate in periods corresponding to the rate of vibration of the source. We may term this power of receptivity sympathetic vibration, or the sympathetic state, and this we must examine now in greater detail. Here I have a pendulum which is capable of swinging to and fro in a definite time. If now I bring that pendulum to rest, and then blow it with my breath, I can set it in rapid vibration by properly timing the impulses which I give to it. By the side of this pendulum is another, which is capable of vibrating at precisely the same rate. If I bring one of these pendulums to rest, and set the other in motion by its side, it is evident that the to-and-fro motion of the pendulum will produce motions in the air and wooden framework which will be precisely timed to the motion of the first pendulum (Fig. 2). Hence, after a certain time, we shall find that the to-and-fro motion of the one pendulum will set the other into vibration. If, however, I shorten one of these pendulums, so as to make its rate of vibration not coincident with that of the other one, then the motions will not be so timed, and the second pendulum will not be thrown into motion. You may have illustrations of this transference of motion in various other ways. Here, for example, I have two strings which are tuned in perfect unison, and attached to the same soundboard. If now I put this first string into motion, it will communicate its vibration through the wood to the second

string, throwing that also into motion, and the motion of the second string will be evident to you by its throwing off these little paper riders. And what is more remarkable still is that the vibration communicated to the second string will be subdivided as in the first string. This subdivision will be taken up by the second string, in precisely the same manner as it exists in the first.

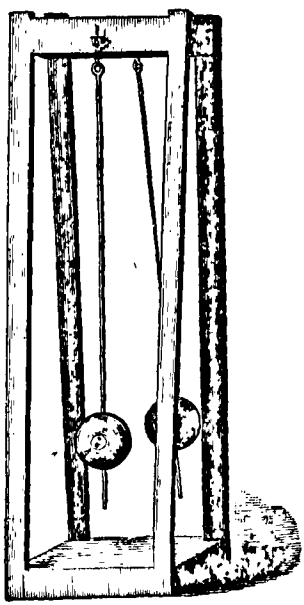


FIG. 2.

These then are cases of the transmission of vibratory energy through solid bodies, and the acceptance of it by other bodies tuned to the same pitch as the first.

Here is an illustration, however, of a more remarkable kind. We have here a tuning-fork which is capable of vibrating at precisely the same rate as this other tuning-fork which is in my hand. I will place one of these forks in front of a lamp, and in order to enable you to see the experiment more clearly, I will cast a shadow of it on

this white screen. Now I strike this first fork, and the vibration will be transmitted through the air to the other fork, against which a bead is placed. You will find that the second fork is thrown into motion by the motion of the first fork. This is remarkable, inasmuch as we have the transference of motion through the air to this very heavy massive fork. If, however, I attach to one fork a little wax it will be thrown out of tune, and I shall not be able to obtain this response, for it now vibrates at a rate slightly slower than before, and we shall find that it will not affect the other fork. There is no trace of vibration.

These are illustrations of the acceptance of vibrations by gross matter.

The same law holds true, as you are aware, with regard to the finer matter composing the ether. If I move this magnet to and fro, it is capable of setting the little magnet below it also in motion to and fro, and this action takes place although I may have one of the magnets in a vacuum, so that it is very evident that the vibration of the magnet is not communicated to the other magnet by means of gross matter. It is communicated by means of something which is finer than the matter which we can weigh—the insensible matter which we denominate ether.

A more striking and beautiful illustration of this fact is given in the following experiment. In front of a soft iron rod surrounded by a coil of wire, a bar electro-magnet, in fact, a magnetized tuning-fork, is firmly fixed. Wires lead from the coil to a second similar arrangement at a distance. On now bowing the first fork, which is mounted on a non-resonant surface, the distant fork, mounted on a resonant box, is heard to sound. The two forks are exactly in tune, and the approach and recession of the magnetized prong to the coil of wire generates electric waves, which are converted into magnetic pulsations in the second fork, the rhythmic rise and fall of which throw it into corresponding vibration. But if one of the forks be loaded the response ceases, for there is no longer synchronism throughout the system.

In like manner heated bodies emit vibrations which are accepted by other bodies if the rates of vibration of the cool substance coincide with the rates of vibration in the hot ones. This fact was pointed out long since by

Professor Balfour Stewart, who showed that although rock-salt is a substance extremely transcalent to ordinary rays, yet it is remarkable for the absorption of the rays emitted by itself. A plate of warm rock-salt gives a radiation which is entirely absorbed by a plate of cool rock-salt, although the cool rock-salt would transmit a very large percentage of any other kind of radiation. This fact may be also seen in the various experiments which are more or less familiar to you all. If you take a tile, or a porcelain plate, on which there is a black pattern on a white ground, and heat that plate to whiteness, the pattern becomes reversed. The black portions of the plate which absorb most light are also capable of giving out most light; and hence they appear light upon a dark ground. A still simpler experiment is to take a piece of platinum foil and write upon it with ink. If then you heat this platinum foil to redness, the word which you have written upon it in ink appears to be bright upon a dark ground; that is to say, the black ink which absorbs most of the radiation is also capable of emitting most, and hence the word appears brighter than the surrounding platinum. You also find that the appearance behind is reversed. The greater radiation from the blackened surface on the platinum robs the platinum of some of its heat, and hence, although the word appears to be bright in front, yet, owing to the loss of heat by radiation in front, the platinum appears on the other side darker where the word is written than in the surrounding part.

Here, then, we have the reciprocity of radiation and absorption shown in the case of molecular vibrations; and this reciprocity is, as you are aware, the foundation of spectrum analysis. For example, cool sodium vapour absorbs the radiation of glowing sodium vapour. You are all probably aware of the names which cluster around this generalization of Kirchhoff. It has, however, always seemed to me that the name of Professor Tyndall ought, in justice, to be associated with the earlier workers at the law which underlies the subject of spectrum analysis. At the period of Kirchhoff's discovery, Professor Tyndall was at work on the absorption of heat by gases and vapours. The apparatus which he employed is exhibited in the Loan Exhibition, and I have it here. He used as a source of

heat either boiling water, or a heated platinum spiral, or the flame of a lamp. This heat he transmitted through a long experimental tube, the ends of which were closed by plates of rock-salt, and which tube could be exhausted by means of an air-pump. The radiation from the source passing through the exhausted tube and plates of rock-salt practically unhindered, fell upon the blackened face of a thermopile. This pile was associated with a galvanometer, and the deflection produced on the galvanometer by the heating of the anterior face of the pile was compensated by warming the posterior face of the pile, and so the needle of the galvanometer was brought to zero. If now into this exhausted tube an elementary gas were introduced, such as oxygen or hydrogen or nitrogen, it was found that little or no difference in the flux of heat through the tube was indicated; whereas if, instead of an elementary gas, a compound gas, such as olefiant gas or ammonia, or any other of the many compound gases, were used, a very great diminution in the flux of heat was perceived. It was greatest in the case of the most complex gases, and least in the case of the least complex gases. Professor Tyndall, however, went much farther than this. He examined the radiation as well as the absorption of these gases—the radiation obtained by heating the gas and allowing it to stream up in front of the thermo-electric pile, and the result showed that the radiation from the elementary gases was feeblest, the radiation from the compound gases being found most abundant. But by another method he examined the subject with still greater care. Doing away with all sources of heat, and dividing the experimental tube into two parts, separated by a plate of rock-salt, he exhausted both chambers. Into the chamber most distant from the pile he now allowed a gas to stream. When an elementary gas was thus allowed to enter the exhausted tube, the collision of its particles against the side of the tube caused a conversion of motion into heat. The particles of gas were warmed, and radiated their heat through the exhausted portion of the tube and to the pile. This Professor Tyndall termed "dynamic radiation." If now a compound gas were allowed to enter this distant portion of the tube, after it had been once more exhausted, a still greater radiation was perceived, just as in the case when the

gases were artificially heated. Now he went a step farther. The portion of the tube nearest the pile, instead of being allowed to remain empty, was successively filled with different gases, and it was found that the elementary gases, which are so transcalent to ordinary heat emitted from lamp-black or glowing platinum, are extremely opaque to the radiation emitted from themselves. That is to say, if into this near portion of the tube oxygen gas be admitted, and then oxygen be allowed to stream into the hinder exhausted portion of the tube, the dynamic radiation from this portion of oxygen is completely intercepted by the absorption of the particles of oxygen in this other portion of the tube. And still more strikingly was this seen when, instead of an elementary gas like oxygen, a compound gas like olefiant gas was admitted into this part of the tube. Then a mere trace of a compound gas of the same nature in the front part of the tube completely intercepted the radiation in the posterior portion of the tube. Further, if we use here, as a source of heat, the radiation from a hydrogen flame which generates water or aqueous vapour by its combustion, then the radiation from that incandescent aqueous vapour is entirely intercepted by a small trace of cool aqueous vapour present in the tube. In like manner, if we use the radiation from a flame of carbonic oxide which produces carbonic acid by its combustion, the radiation from the glowing molecules of carbonic acid is completely intercepted by a small trace of cool carbonic acid in the experimental tube.

In this way I have found that we may make an extremely delicate analysis of the human breath. By introducing traces of the dry breath into the experimental tube, and using the radiation from a carbonic oxide flame, we have the means of making a physical analysis of the breath, exceeding in delicacy and even rivalling in accuracy the ordinary chemical methods.

You will perceive that all this is really an illustration of Kirchhoff's familiar law of the reciprocity of radiation and absorption. Hence I think you will agree with me that Professor Tyndall, who was led to these conclusions by an independent line of research, if not a pioneer in this direction, like Stokes and Stewart, yet should in truth be regarded as one of those who contributed to lay the broad

and firm foundation on which Kirchhoff's law now securely rests.

But supposing the vibration be not a simple but a compound one,—that is to say, the coalescence of many and varied vibrations—will such a complex or intricate wave-form be able to set a body in motion? It can do so by virtue of the fact that, however complex the wave-form may be, it can be resolved into a series of simple pendular vibrations. If a body can respond to one of the constituents of the compound tones, a feeble resonance or sympathetic vibration will be produced. Thus, if we take a bundle of tuning-forks of different pitch, and sound them together, they will generate by the coalescence of their sounds a compound tone, which, falling upon a certain silent fork, will set that fork into motion if the particular period of vibration of the silent fork be found among the constituents of the compound tone. That you have already seen in one form. It would have been very evident to you if, instead of striking one tuning-fork, I had struck twenty or thirty with notes of different pitch. Only one of those forks would have been concerned in setting the silent fork in motion—namely, that one which vibrated in exactly the same period as the silent fork. Helmholtz has rendered it extremely probable that this is the manner in which we are enabled to distinguish the mixed multitude of sounds in an orchestra. A structure in our ears called Corti's arches, is attached to an elastic membrane called the *membrana basilaris*, the particular tension of which appears to tune these Corti's arches so that they are capable of responding to notes of different pitch.

We have shown the effect of synchronism on the reception of sympathetic vibration, so that if we have a Corti's arch capable of responding to a certain tone it will be thrown into violent vibration by a corresponding note. The neighbouring arches will be thrown into less vigorous motion, and thus we may imagine these structures tuned, as it were, to notes of different pitch, as, indeed, Helmholtz has shown to be extremely probable. The investigations of Mayer have shown that insects seem to possess an analogous mode of hearing by the reception of vibrations through filaments attached to their bodies.

Now, so far we have seen the acceptance of vibration by

solid bodies. If we take a lighter medium, such as air, a more vigorous and prompt reinforcement of the sound is produced. At the same time the decay of the sound is more rapid on the removal of the source. Here I have a tuning-fork. When I strike it and hold it over a particular column of air, the air within the jar responds very vigorously. The reinforcement of a note of definite sound in this manner is termed *resonance*.

If I pour water in the jar, thereby obtaining a column of air vibrating at a different rate, the response will not be nearly so loud. Of course by decreasing the length of the column of air we shall increase the difference between the two vibrations. If now, in place of a simple tone, as in this tuning-fork, I produce a compound tone, the jar will pick out from the compound tone that one constituent which corresponds to its own rate of vibration, and will reinforce that tone to the exclusion of all the others. This is the principle of Helmholtz's resonators which are capable of responding to notes of a certain definite pitch; that is to say to simple pendular vibrations. If the nozzle of one of these resonators be placed in the ear, and a compound tone be sounded, the resonator will respond to one particular tone only, and thus it can be ascertained if that tone be among the constituents of the compound tone. Helmholtz has thus made an analysis of vowel sounds by means of these resonators.

I have here a series of brass pipes, which are of different lengths, and on pulling out one of these pipes you hear a little explosion. That explosion generates an extremely intricate wave-form, inasmuch as it is produced by the coalescence of a great many vibrations of different periods. Nevertheless, this column of air within the tube can respond to vibrations of only one period. Out of the multitude of vibrations existing in the explosion which follows the sudden withdrawal of this tube, the air within this tube selects one tone and reinforces it, and thus you get, quite audibly, the note of the tube by the sudden pulling out of the stopper. Inasmuch as these tubes are tuned so as to give notes of definite ratio, I think we shall find that from the noise made by simply pulling out these tubes successively we may get the notes of the common chord.

This is an explanation of the sound given by organ pipes

where the whistling or rustling of the air gives rise to a multitude of sounds, only one of which is strengthened by the resonance of the air within the pipe. In like manner this affords an explanation of the so-called singing flames. Instead of using air urged through a narrow orifice, we have gas urged through a narrow orifice; which itself produces a slight noise or rustling sound; and further the combustion of the gas gives rise to a rapid current of air within the tube which is placed over it. The tube around the flame is capable of responding to one note, and only one; and hence by proper adjustment of the tube with regard to the flame, the flame will sing or produce a continuous musical note. With a longer tube you hear a lower note produced, owing to the fact that the resonance of this longer tube responds to a note of lower pitch. The tube may be easily tuned to a note of a definite pitch by a paper slider on the end of the tube. Wheatstone has, indeed, made a flame organ on this principle. Here is the instrument from the Loan Exhibition. By depressing the keys of the instrument certain gas jets are suddenly thrown within their tubes, and the tubes corresponding to these burners then begin to sing. Thus you can play a few chords upon an instrument of this kind.

It has long ago been shown how these singing flames could be modified by simply heating a piece of wire gauze which was placed within the tube. If the wire gauze be made red-hot, then a broken current of air passes up the tube. The air passing through the wire gauze thus produces a slight rustling sound in the tube. Out of these notes the tube selects the proper note belonging to itself, and reinforces that note, and by the resonance of the air in the tube we have a musical sound produced. I will warm the wire gauze by a flame. You will see in this case that when the tube is held horizontally, the current of air and the sound cease, but when the tube is vertical the note reappears.

We must pass on to the last fact in this chain of phenomena. The body receiving the vibration and accepting it may be in such a peculiar abnormal state that a slight disturbance will produce a great and disproportionate result. And we may term this the *sensitive state.*

Various illustrations of this will at once occur to you. A stone poised on the edge of a cliff, for example, can be thrown off by a series of timed puffs—puffs recurring at the rate of vibration of the stone may cause its overthrow, and thus a great change may be brought about. Prince Rupert's drops exhibit this sensitive condition. Certain fulminating powders also exhibit this sensitiveness. Or again, if we take a singing flame, and get it nearly at the point of singing, and then sound a note exactly similar to that which the flame itself makes when singing, the flame will be thrown into continuous song.

Now a naked flame may respond without a tube. If we take an ordinary gas burner such as I have here, and bring it near to the point of roaring, then a sound made near that flame will throw it into a roaring condition, accompanied by a shortening of the flame. A slight motion of the flame is perceived. This effect, however, which is exceedingly slight, can be augmented by properly adjusting the weights on the gasholder until we obtain a sensitive flame, properly so called. Here I have such a flame which consists of coal-gas burning from a narrow orifice, yielding us a tall tapering flame which responds to a very slight sound, such for example as the sound of the sibilant.

Now, regarding the history of sensitive flames, it will be sufficient for me to say that the subject was first brought prominently under public notice by Professor Tyndall, who at the same time enriched the discovery that had previously been made. You probably see already the explanation of this phenomenon. The flame accepts the vibration which it can itself emit. If, for example, we light this flame, and now put a little pressure upon the gasholder, you will find that the flame will begin to shorten and roar, just as it did under the influence of sound, making that sibilant noise. A pressure of an extremely slight amount will cause a flame which is on the verge of roaring, to roar. Far less than one hundredth of an inch of water pressure will cause an ordinary batswing burner to turn from a silent flame into a roaring flame. We have, therefore, in a sensitive flame a body in a state of sympathetic vibration, and the vibration of the flame is exactly synchronous to that of the sound which throws it into vibration. The

nature of the vibration can be investigated thus. If a sensitive flame be put under the influence of sound, and a mirror moved to-and-fro in front of it, we shall find the flame exhibiting a state of vibration exactly analogous to that of the sounding body itself. I will make a sibilant sound, and you will see the vibration if I move the mirror to and fro. This is a non-musical sound, but if I make a musical sound such, for example, as that from this reed, a sort of strained and intense divergence of the flame is produced, the vibration of which can be seen better in the mirror.

Now, other bodies besides flame exhibit this sensitive state. Jets of air, rendered visible by smoke, are extremely sensitive to sound; so much so that I have found an almost inaudible sound, made at a distance of two or three hundred feet from such a jet of air, is capable of very considerably affecting the jet. In like manner jets of water can be thrown into this sensitive condition. Savart indeed long ago showed that jets of water could be influenced by musical notes.

Nor need we stop here. The radiant energy of heat and light may produce changes in bodies, analogous to those produced in flames by sound. It has even been suggested that sun-spots—those changes on the sun's surface which appear to be somewhat connected with the approach of certain planets,— may be illustrations of these sensitive flames, as it were, upon a large scale, that is to say, a state of tottering equilibrium, wherein a very feeble agent, if of the proper kind, may produce a profound change in the aspect of a body. Again many present have probably heard of those experiments which Professor Tyndall has made lately, where a beam of light has been sent through a tube containing the vapours of volatile liquids, and where those vapours, acted on by certain rays, suddenly assumed strange fantastic shapes. Such a profound change produced by the radiant energy of light is analogous to the change produced in a flame by the sonorous vibrations of the air. In both cases it is an instance of sympathetic vibration or resonance. And it is not impossible that living organisms, and even the mind of men, may be found subsequently to be subject to a similar law; but into this of course I have no right to enter here.

A few words may be useful to you as to the size and the nature of the jets to be employed; and then I shall show, in conclusion, two or three applications of these sensitive flames. The first form of jet that I employed was a simple piece of glass tubing drawn out to a point, and then filed into a V-shape. Such a jet yields a flame which is capable of being affected by a whistle, and has the advantage of being sensitive under the ordinary pressure of gas. Here is a similar jet made out of a piece of brass tubing. You see that if I whistle to this flame it spreads out sideways. This is at the ordinary pressure. When the pressure of gas increases, as it does towards the evening, the effect is still better. A brass circular orifice also yields a very good sensitive flame under considerable pressure. But the most sensitive kind of flame is obtained by allowing the gas to stream through a circular orifice made of steatite such as is used in the jet photometers. It was, in fact, when noticing the influence of sound upon one of these jet photometers, that I was led to use the substance. The jet photometer burner will yield a very tall tapering flame of gas some two feet high. If the gas be very rich in quality the flame will be much higher than if the gas be poor, and the higher you make your flame the more sensitive it is. Hence a sensitive flame may be employed as an extremely delicate test of quality of gas. You will find that if a burner admits No. 19 wire (Birmingham wire gauge), it requires a pressure of three and-a-half inches of water to bring it to its most sensitive condition. If it admits No. 21, which is a smaller size, it requires six or seven inches of water to bring it to its most sensitive condition. This pressure is best obtained by using the gas from a gasholder and not from a gas-bag. The gas-bag has the disadvantage of continuously varying in its pressure, and, moreover, there are vibrations in the gas-bag itself which are extremely detrimental to the effect sought. The gasholder ought to be one yielding a very steady flow of gas. I have fitted up in my laboratory a gasholder for this purpose.

There is also another point of very considerable importance in connection with the subject, namely, having the gas way perfectly free. The gas-cock should not be partly turned. If it is so the gas in passing through it will be subject

to an incessant ricochetting, and the flame obtained will be less sensitive than if the gas-cock were completely open, and the pressure adjusted by means of weights. The free-

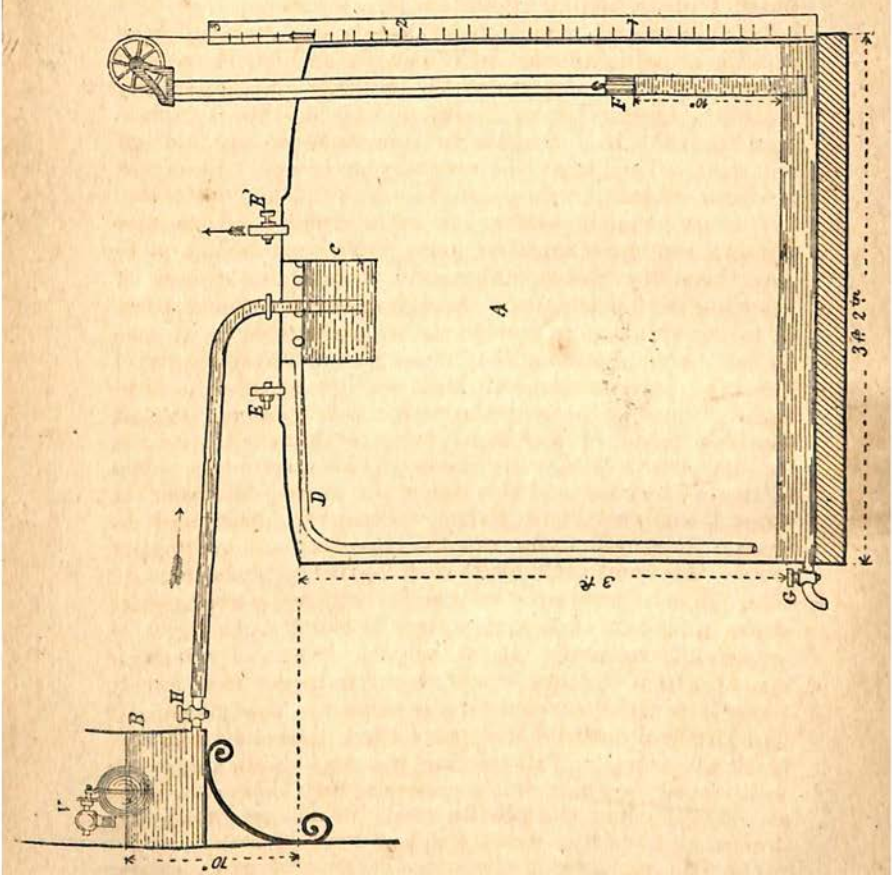


FIG. 3.

dom of the gasway has led me to employ a form of apparatus, which yields the most sensitive flame with which I am acquainted. A perfectly smooth and tranquil flow of

gas is thus obtained, because there is no such cock here, but a completely open way right to the burner.

The source of sound is also important. I have here a ticking watch inclosed in a padded case. I will wind up the watch, and you will find how sensitive this flame is by its being able to respond to the ticking of the watch. The metallic sound being very rich in over tones or upper partial tones, this sound is one to which the flame is particularly sensitive, inasmuch as those notes are contained in the sound made when the flame roars. Further it is the root portion and not the upper portion of the flame which is most sensitive.

Now there remains to me to point out to you a few applications of these sensitive flames. You may use them in detecting the existence of sonorous vibration in the air, or the state of vibration of a sounding body. It is very instructive to examine, by means of a sensitive flame, the vibrations of plates and bells, and so on. You find that you can discover the nodal lines with great sharpness by such means. Here I have an arrangement which can be used as a practical application of these flames, in the detection of sounds, such as a burglar filing his entrance into a jeweller's shop, or it may be used in automatically registering the presence of sonorous vibrations. This little table supports three tubes, one of which carries the flame. One rod carries a platinum wire, and the other a compound riband of silver and platinum. When this compound riband is warmed by the flame, it bends and comes into contact with the platinum wire, and thus closes a battery circuit and rings an electric bell. Every time, therefore, the flame is under the influence of sound, it will shorten and spread out sideways, and thus touch the compound riband, and thereby closes the circuit and rings the bell. I have usually a glass cylinder surrounding this apparatus to keep it from the surrounding draughts.

It is extremely difficult in a class to show the ordinary laws of reflection and refraction of sound, but a sensitive flame enables us to do so with great ease.

LIGHTHOUSE ILLUMINATION.

BY PROFESSOR T. F PIGOT

THE subject we have to take up to-day is that of light-houses, or rather the illuminating portion of lighthouses, with a description of the apparatus used for this purpose, which is exhibited in this collection.

Lighthouses have for their object to direct the course of vessels at night, sometimes warning them off dangerous points, whether reefs, rocks, or sandbanks, sometimes guiding them in the direction of a channel they have to follow.

Hence it will be at once clear that light thrown upwards, above the horizon, and light thrown downwards, towards the base of a lighthouse, would be of no use whatever.

In lighthouses built on land, the land side does not require to be illuminated, and even when built on rocks or reefs at sea they are generally not very far from land, and in that direction their illuminating power does not require to be so strong as towards the sea.

Th it is that apparatus serving to utilise the upward and downward rays of a lighthouse lamp by diverting them to the best direction for mariners must be advantageous; and further, if by any means, the rays towards the land can be utilised for strengthening the seaward light, or in giving greater intensity of light to certain required arcs in azimuth, say in the direction of a channel, there will be a gain to mariners of all that light which would otherwise be wasted.

In those cases, when certain arcs in azimuth require less light than others, a part of the light thrown upon them might be usefully spent upon the remainder of the circumference of light. The apparatus invented to meet these various requirements, and which I am about to describe, have reached such a pitch of perfection, that it may be almost laid down that all the light from a lighthouse lamp can be projected in whatever directions the constructor may desire.

Before entering on a description of the apparatus I shall at once state that all the information upon these subjects, as far as I am aware, is to be found in the works of Messrs. Alan and Thomas Stevenson, and in that of M. Reynand, of the French Ponts et Chaussées Department. These works are Mr. Alan Stevenson's description of the Skerrycore Lighthouse, and one called "Lighthouse Illumination," by one of the family. M. Reynand's is on the lighting and buoying of the coasts of France.

Messrs. Stevenson, following in Fresnel's footsteps, have reached by their inventions, coupled with his, the extraordinary perfection to which I have alluded. All the calculations of the forms of Fresnel's catadioptric rings, and of Mr. T. Stevenson's new forms of prisms, will be found in the works I have alluded to, and I shall only describe the models before you, their principle and object, with the aid of a few diagrams (enlarged from Messrs. Stevenson's works), without attempting to enter into their calculation, which would supply by itself materials for a course of lectures.

The first historic lighthouse may be said to be that of Corduan on the Garonne in France, erected nearly 300 years ago, and about 200 feet high, first lighted by a wood fire, then in 1784 by Lenoir's lamps and paraboloidal reflectors, and in 1822 by Fresnel's dioptric instruments. Then come in order other historical structures, such as Smeaton's Eddystone Lighthouse, twice destroyed by fire, and the Messrs. Stevenson's Bell Rock and Skerrycore lighthouse, the description of the erection of this last by Mr. Alan Stevenson himself being one of the most interesting records of ability and perseverance in the annals of civil engineering. It was for this Skerrycore light that the first great improvements in Fresnel's apparatus were

undertaken, and here is the place to describe Fresnel's original models and apparatus which are here before you. I should mention here a little discrepancy of terms in various authors. In general, reflecting lights are called catoptric; such as are not reflected but are refracted, where lenses are used, are usually called dioptric lights. Lastly, what I shall presently speak of, certain rings separate from the interior lens, are called catadioptric rings. Mr. Stevenson has introduced into one of his books the term catadioptric for a combination of reflection and refraction.

Up to Fresnel's time paraboloidal reflectors were employed, consisting in a paraboloidal mirror of polished metal (generally silvered copper) or composed of facets of glass as in this model. At that time it was extremely hard to obtain glass of the requisite curvature, and they were composed of these small facets. There is a great advantage in having glass over metal because there is a great deal more of the light reflected, and not lost. A lamp is placed at the focus of the mirror, and its rays striking the mirror are reflected horizontally outwards.

The loss of light from highly polished reflectors is considerable, varying from ·31 to ·35 of the entire beam, when the whole amount is taken as unity, and from lighthouse reflectors which cannot be kept at a very high polish it is even greater, being about ·444, or very nearly half. But besides this it will be seen that a large proportion of the rays from the lamp are cast upwards and downwards beyond the limits of the reflector, and are thus completely useless; but even with this loss this reflector gave so much light that it was used for a great many years, and is used still. If a number of such reflectors and lamps are placed side by side, either on a polygonal or circular framing, and several tiers, placed one above another, a bright light is produced. The polygonal form is for a revolving light, the circular for a fixed one. This system is still employed in many lighthouses, and such reflecting apparatus are called catoptric.

In his dioptric apparatus Fresnel placed round one single central lamp a series of planoconvex glass lenses (such as you see in the central portion or drum of this model), which, by their well-known property, refracted horizontally the rays of light from the lamp, and in order

to give these lenses a sufficient diameter, he further constructed them in the form known as that of echelonned lenses, invented first it is said by Buffon, and afterwards independently by Fresnel himself. We have here the original lenses, constructed by Soleil, the great optician of his day in Paris, under Fresnel's orders. You will perceive that the larger lenses are square, the corners being made up in a similar manner by portions of an echelonned lens.

Fig. 1 represents one of these lenses in horizontal

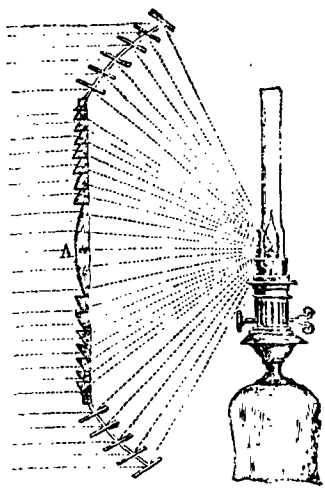


FIG. 1.

section. You will notice that the form of the lens is produced by revolving a section round a horizontal axis. It is the form adopted for revolving lights, when the lantern is of polygonal form. As the lantern revolves each time that a face passes any particular direction in azimuth, a flash is emitted through the lens, followed by almost total darkness when one of the angles comes round into the same direction. These lenses are set in frames slightly oblique to the frame of the lantern, so that in the case of a complete beehive, such as this, the flash is prolonged, the

upper and lower portions (of which we will now speak) not being exactly directed towards the same arc in azimuth with the central drum. In the splendid light exhibited in the grounds outside this collection, constructed by Messrs. Chance Brothers for the Little Basses lighthouse, Ceylon, another arrangement of the lenses is adopted. It is like one of these complete lenses cut in two, and the two halves given a slight angle, so that there is a double flash.

To utilise the light passing above and below these lenses, Fresnel invented what are called catadioptric rings or zones. These he only used for small lights of what are called the fourth order, and it remained for Mr. A. Stevenson to apply the same principle to the larger lights. Before proceeding with this I shall say one word with regard to the manner in which catadioptric rings are formed. There is in every transparent substance what is called a critical angle. If a ray passing through one face of a prism, meets another face at an angle less than the critical angle of the material, that ray is totally reflected, and no refraction takes place.

The paths of the two extreme rays for each prism really determine its form. Of course everything connected with the direction of these rays depends upon the index of refraction of these various glasses, of which flint glass is the highest. I should mention that the curves there, instead of being the true curves, found by formulæ, are always made circular, and the other sides are either straight, or one slightly concave, and the other convex. The ray passes on to meet the prism, and a similar calculation gives its form. The prisms you see in these beehive-formed lights are constructed on this system, and the lower prisms are similarly calculated. With this arrangement all the light is utilised, and sent out in as many parallel groups of rays as there are sides to the polygonal lantern or beehive. When only a fixed light is required, which is to be seen from all points of the compass, the polygonal form would not answer, as the arcs in azimuth near the angles would be in darkness. For this purpose Fresnel adopted a circular lantern, the lenses of prisms being generated by the same section, but revolving round a vertical axis passing through the centre of the lamp. Here is one of that form of the fourth order

of lights. For large lights Fresnel employed ranges of lenses below and above the central drum which parallelised upwards and downwards the rays from the lamp which were there reflected horizontally by corresponding paraboloidal mirrors.

I must not omit to refer to Fresnel's first catadioptric apparatus used for lighting the canal St. Martin, constructed in 1825, and to this wooden model for another similar but larger one. There is also exhibited here his first lens, polygonal in form, in consequence of the difficulty at that time of executing lenses in glass. Here are besides two other lenses, one polygonal, the other circular, for lights of the first order (this one being in 100 pieces).

Before proceeding further I shall here notice the various improvements in reflector lamps. The portion of the paraboloid behind the focus is replaced by a spherical mirror which throws back the light to the flame to be transmitted to the front. The upward and downward rays are caught by a lens whose position is determined so as to catch the rays from the lamp which would not meet the mirrors, without interfering with the action of the reflector. The lens can be placed either inside the reflector or altogether outside, leaving its centre open to let the rays from the reflector pass through.

M. Bordier Maret invented a double reflector sending light in two opposite directions, composed of two paraboloidal reflectors, back to back, with a common focus. If this section is made to revolve round a vertical axis passing through the common focus, a lantern at this focus will send its rays horizontally to all points of the compass. A paraboloidal mirror revolving round the lamp gives a flash at every point in azimuth in one revolution.

I shall now describe the various improvements invented and put in practice by Messrs. Stevenson. I shall first take up the case of mirrors. Messrs. Stevenson employed in 1814 the parabolic mirror with a lens in front to catch the rays diverging beyond the reflector or mirror, and with a spherical mirror behind the lamp, so as to turn back the rays to the focus, and by these means he utilised almost all the rays. Subsequently he invented a glass spherical mirror of which Fig. 2 is a section. The diagram on page 208

shows the path of the rays and the form of the section. The ray enters normally, and therefore without loss, it is totally reflected at r' and returns again without loss. There is a circle whose radius is the lamp f; where parabolas whose common focus is F ought to be used. They are commonly made circles, osculating the parabola. The whole section is then made to turn round a vertical axis. The rings are now made separate from each other, a form adopted by Mr. Chance of Birmingham, and approved of

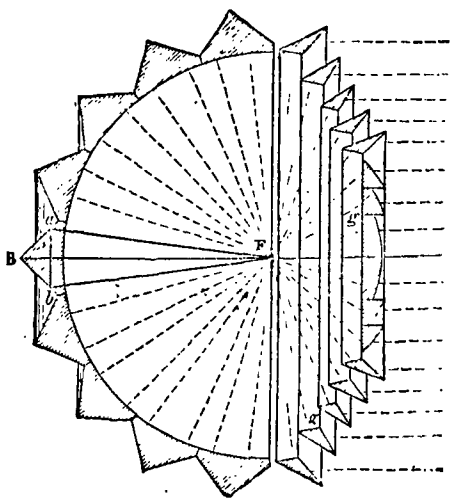


FIG. 2.

by Mr. Stevenson. The great advantage in these mirrors is in the diminished loss of light by reflection, the loss being only about ·230 in place of ·444, the loss in metallic mirrors. The apparatus you see here is what is called a dioptric holophote. It is composed of one of these glass totally reflecting mirrors, with an echelonned lens in front. When in front of such an apparatus straight prisms are placed of the form indicated in the section (Fig. 2), the rays are all collected into foci, as at $f'f'$, and then diverge into an azimuthal angle due to the number of prisms, thus the light will be seen in a vertical strip of the breadth of

the prism *c c*. This system of prisms employed by Fresnel for flashes may be used for increasing the divergence and intensity of light in certain arcs of azimuth.

We now come to what is called condensing apparatus, which consists in utilising the useless rays which would otherwise fall on the land, or on some parts of the horizon where they are not wanted. This apparatus which I shall now describe is a fourth order condensing apparatus constructed for the Lamlash lighthouse. Fig. 3 is a vertical section of the prisms and lenses of the Lochindaal condensing apparatus, and applies equally to the Lamlash model. *A B C* is a half beehive on Fresnel's system. In this case the angle in azimuth to be strengthened is of 82°.

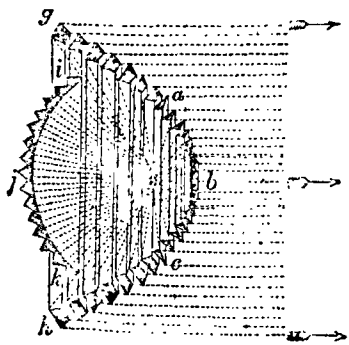


FIG. 3.

The rays from the central Fresnel beehive over the arc *b a* are diverted by vertical prisms shown in section at 1 to 16, and, to take up less space, some of them, namely 10 to 14, are placed behind the others, and room is made between the front prisms for their rays to pass by; using twin prisms such as from 4 to 9 to give more space. The rays from the whole semicircle, *abc*, are thus utilised on the arc *ab*; for the most divergent rays of prisms, 1 and 14, are nearly parallel to the radial lines *fa*, *fb*. The half not represented of the circle *abc* is completed by a dioptric spherical mirror, which sends back all the rays striking the focus *f*.

It will be remarked that the central zones of these mirrors are close together, the upper and lower are also

spherical, but of increased radius; this is what gives the irregular appearance to the back of the mirror. By the construction of all these apparatus the light from the lamp is all utilised for illuminating an arc in azimuth of 82°.

The last of these lights I have to describe is the fixed azimuth-condensing light for Buddonness at the entrance of the Tay, to illuminate an arc of 45°. It is in the collection, but the model is too large for transport. I have here vertical and horizontal sections showing the lenses and prisms employed. There are nowhere above four refractors and four total reflections of a ray. Five optical agents are employed in the construction. As an example of the utility of this I will give you a rough plan of the place it was intended for, namely, the Isle of Oronsay near the

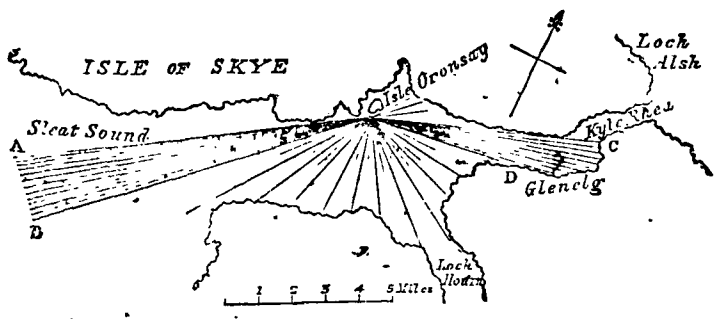


FIG. 4.

Isle of Skye, where the light is utilised for guiding ships along two channels. I must now hurry on to say one word about a highly important part of illumination in lighthouses. You have often buoys and beacons placed in harbours, some of them almost unapproachable, and where it may be difficult or impossible to erect a lighthouse; hence what are called dipping lights have been employed. A lighthouse on the adjacent coast contains not only its own summit light, but also a light from a window, as represented, dipping towards the direction of the reef or rock, and of such divergence that when a ship comes within range of it, mariners will know that they must change their course. Sometimes in place of a window light, part

of the light of the upper lantern is collected by lenses below it, and reflected out in the required direction. If there is danger of confusion between the two lights, the upper one can be white, the reef light red. When the land is low and the danger distant the dipping light would be insufficient for protection, and another course has been resorted to, namely, that of apparent lights. One of these has been erected at the entrance of Stornoway Bay with perfect success. The beacon is rarely accessible. Its light is thrown from a window of a lighthouse in a strong horizontal beam on a mirror on the beacon, from which it is

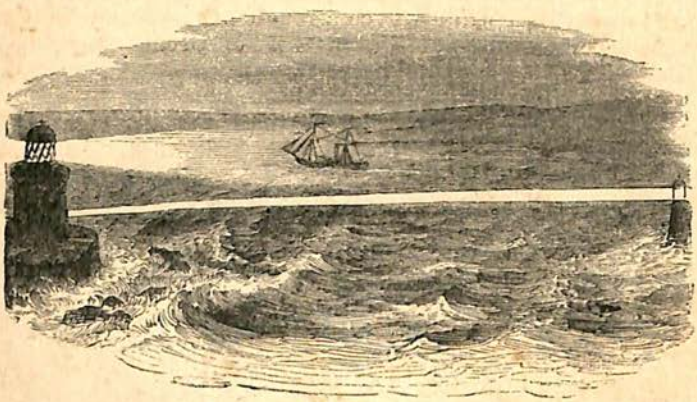


FIG. 5.

reflected through a lens so as to diverge in the required direction for ships entering Stornoway Bay.

The silver of the mirror gets injured by damp, but after attempting hermetically sealed lanterns it was found necessary to unseal them as the sea air got in through the putty, and caused a haze over the glasswork. Mr. Stevenson's new prisms have been used for the same purpose, but in that case this prism serves instead of the mirror and lens.

In spite of all these improvements there appears little doubt that a good reflector, with a holophote such as I described before, gives excellent results. Mr. Stevenson recommends that the metal section of the reflector shall be parabolic, the horizontal section hyperbolic, or elliptic,

so as to give a certain amount of divergence (without interfering with the holophote for the central cone of rays). He recommends mirrors of silvered glass fixed together by Canada balsam, a substance which has nearly the index of refraction of plate glass. I have now given a very brief description of these beautiful apparatus, and if I had time I should wish to give some description of the system employed for insuring a steady burner for the lamp, but I believe you will hear this portion of the system described by one much better skilled in the subject than I am. I will, however, just direct your attention to the concentric wicks employed for burning oil; the transparency of flame rendering the internal portion available. The mechanism for keeping up the supply of oil is an ordinary clockwork. Now the electric light is often employed in place of rock oil and paraffin. In all cases when necessary it is possible to vary the colour of the light by colouring the lenses and prisms in the portions required. Red is the ordinary colour used when a second colour is required, but other colours may be useful for purposes of distinction.

On the table are examples of lamps for ship's sides of various colours on Fresnel's principle.

THE VELOCITY OF LIGHT.

BY PROFESSOR FORBES.

AMONG the instruments that may be seen in the Loan Collection at present there are many that have been used in those great researches that have laid the foundations of physical science. It is important as much for those who are engaged in teaching science as for those who investigate it to study these instruments, to note their merits, to observe their defects, to judge how far we are to accept the results as conclusive, and to reconcile the contrary results sometimes obtained by different observers, owing to their having employed different forms of apparatus. It is also important that those engaged in teaching physics should have the opportunity of coming in contact with these instruments, and learning from those who have been employed in such investigations the peculiarities of the instruments, which can be learnt only by experience.

Having been lately employed in experiments on the velocity of light, I have been honoured by the request that I should open this course of lectures with an account of what has been done in this important inquiry, and I willingly undertook the task, because the means of experimental research employed are as instructive as they are ingenious.

This is a subject which might at first sight seem to be beyond the scope of man's inquiry. But the problem has been attacked by the most skilled experimenters, and has been solved to an extent that does credit to the ingenuity of the methods employed.

The ancients had very vague notions about the nature of light. They supposed that something was sent out from the eye which acted the part of a feeler, to inform a person of the position of different objects. This is quite opposed to all our knowledge of physics. The true theory could not fail to gain ground so soon as it was enunciated, viz., that something passes from luminous objects, and either strikes our eyes directly, or may do so after having first been intercepted by other objects. Thus we obtain an idea of the existence of luminous and illuminated objects. What is this something that passes to the nerves of the eye? This question has been answered by the two theories that were for a long time rivals; the *corpuscular* theory supposed that it was matter in a finely divided form that performed this function. The *undulatory* theory, on the other hand, assumed the exciting cause to be an undulation like the waves of the sea, that are propagated through a medium filling all space, and permeating the mass, at least of all transparent bodies. It is not my purpose to-day to explain to you the number of proofs that combine to render this theory more than probable, but we shall see how the experiments about to be described settle the question between the corpuscular theory, as enunciated by Newton, and the undulatory theory. This last was first propounded by Huyghens in 1690, and was taught in the following year at St. Andrew's University by James Gregory. But the full expounding of it was done chiefly by Fresnel and Young at the beginning of the present century.

But let us return to the question of the velocity of light. The first man who gave reasons for believing that light takes a sensible time to pass through space, was the Danish astronomer Roemer. He found at his observatory at Copenhagen that the eclipses of Jupiter's satellites could not otherwise be explained than on the assumption of a uniform revolution of these satellites round the planet. Moreover, he made the curious discovery that this inequality depended on the position of the earth, and that in consequence it could hardly be due to any real inequality of motion of the satellites round the planet. The fact that he noticed was this, that when Jupiter was on the side of the sun away from the earth, the eclipses were later than the predicted time, and when on the same side they were earlier. Now

in the first case the light has to pass over a distance greater than in the second case by a quantity that may amount to the whole diameter of the earth's orbit. This will be easily understood by a glance at the diagram. This is exactly the kind of effect that would be produced on the assumption that light takes a sensible time to pass over space. Roemer found that the maximum difference of time was about eighteen minutes. At that time there was no knowledge about the size of the earth's orbit, except from some ingenious speculations of the accomplished Jeremiah Horrocks. But with our present knowledge of the distance of the sun, this would indicate a velocity of 310,000 miles a second.

On reading the scientific literature of that day we find that there was a general scepticism prevalent as to the brilliant discovery of Roemer. It was not until Bradley announced his discovery of the aberration of light, in 1728, that men of science generally accepted these views. Bradley was employed at that time in making accurate observations on the star γ Draconis, to attempt to find a change in the place of the star produced by the change of position of the earth in her orbit, which had been announced as observed by Hooke. In technical language, he was trying to measure the parallax of the star. He chose this particular star because it passed so near the zenith, and so the errors of refraction were reduced to a minimum. He certainly found an annual change in the position of the star. But it was produced at a time when parallax could not produce any effect.

To understand how the gradual propagation of light could produce such an effect, let us look at the diagram. If the earth remained fixed in the position E, the star S would of course be seen in the direction E S. But if the earth move over the distance B E, while the light from the star passes over the distance A E, then the star will be seen in the direction E S′, for that is the direction of the ray of light *relatively* to the earth. An analogy will make this clearer. In a steady downpour of rain, if you are standing still you hold your umbrella upright; but if you are walking fast you incline your umbrella forward to catch the rain drops. So when the earth is moving in a direction at right angles to the ray of light coming from a star, you

must incline your telescope forwards to catch the ray of light, that is to make it pass along the same path in the telescope. The amount that the telescope has to be moved is the angle S E S′. This explanation is quite clear on the corpuscular theory, but the same explanation has been shown by Stokes to apply to the undulatory theory also.

Bradley called this source of error the *aberration of light*, and found that it amounted to 20″. The tangent of this angle, or $\frac{\text{E B}}{\text{A E}} = \frac{1}{10210}$, hence, from Bradley's observations, the velocity of light is 10,210 times the velocity of the earth. Now the distance of the earth from the sun is about 92,000,000 miles, hence the circumference of the earth's orbit is this quantity multiplied by 6·28318; and this distance is accomplished in 365¼ days. This gives us 19 miles a second as the velocity of the earth, and 194,000 miles a second as the velocity of light.

A more accurate value has since been obtained by Wilhelm Struve, and since confirmed, of 20″, 445. This gives us a proportionally altered value for the velocity of light.

The results which had now been obtained, while they were sufficient to give a fair idea of the amount of the velocity of light, still left room for much more important work. In both the methods hitherto spoken of, the velocity of light was calculated from an assumed knowledge of the distance of the sun from the earth. But you will easily see that this is a very difficult thing to determine, when you remember the expense and trouble that was spent in 1874 to find it more accurately by the method of the transit of Venus. It was an important object then to measure the velocity of light by some independent means. This difficult problem was first attacked by M. Fizeau; and with perfect success.

We have before us the apparatus employed in this splendid investigation. The problem seems hard enough. A beam of light was to be sent from the observer's station to a distant reflector, and the time occupied before it returned was to be measured. From the figures I have already given you, you will see that this must be a very small fraction of a second. The method employed by M. Fizeau was extremely ingenious. Here is a disc that is blackened to prevent it from reflecting much light, it has

round its circumference a number of notches, like the teeth of a cog-wheel. This wheel is 4 inches in diameter, and there are 1,000 notches, or teeth, in its circumference. By the mechanism which you see appended, a great velocity can be given to it.

The principle upon which this acts is very simple in theory, though it requires a little care in the practical working. The object aimed at is to send a beam of light past the edge of the disc, so as to pass through a notch between two teeth. It is then to pass over a considerable distance to a reflector, from which it is sent back. But on reaching the revolving disc it may happen that the space between two teeth through which the light passed before, is now occupied by a tooth, so that the light is not allowed to return, so that an observer will not see the reflected light if the disc be rotating at such a rate as to bring a tooth into the position previously occupied by a space, after a time equal to the time taken by a light to pass over the distance to the distant reflector and back. It is thus clear that if the experimental arrangements were properly carried out, the observer would see no return of light. If, however, the disc were rotated still more rapidly, it might happen that in the time taken by light to pass over the given distance the disc has revolved so that the return light passes through the notch next to the one through which it passed on its outward start. Thus, as we increase the rate of rotation of the disc, we get alternately a full return of light and complete darkness.

Here we have the apparatus by means of which Fizeau overcame the difficulties of the problem. The revolving disc is placed in the focus of a telescope, the eye-piece is detached, and between the disc and the eye piece there is an inclined piece of glass. This serves as a reflector, to send the rays from a light at the side along the axis of the telescope, concentrating them on the edge of the notched disc. From this point the rays diverge to the object-glass of the telescope, whence they emerge parallel. The apparatus is so arranged that the reflecting apparatus is at a distance of some miles, and is seen in the telescope just on the edge of the notched disc, which, of course, is also in focus. Perhaps the most ingenious part of the apparatus is the reflector, which may be described as a

reflecting collimator. It consists of a telescope pointed directly upon the observing telescope. The eye-piece is removed and replaced by a reflector which slides into the place of the eye-piece, and exactly reaches to the principal focus of the telescope. We see now that the rays coming parallel from the observer's telescope, fall upon the object-glass of the reflecting collimator, and are brought to a focus upon the small mirror. The reflected rays diverge to the object glass, and if the instrument is in perfect adjustment, they must retrace their path. Thus they fall parallel upon the object-glass of the observer's telescope, and are brought to a focus at exactly the same part of the notched disc as they emerged from. Thence they fall upon the plane glass, and while some of the rays are, of course, lost by reflection, a considerable part are sent through, and can be observed through the eye piece.

We have now traced the rays of light from the source of light to the distant reflector and back; and I wish to draw your attention specially to three results of this arrangement, which are highly instructive to all those who are engaged on optical experiments.

1. You notice that the rays that pass to the reflecting collimator diverge from a definite part of the notched disc, and after their return are brought back to exactly the same point.

2. The collimator does not require to be perfectly adjusted — a thing that would be impossible, and that renders the employment of a plane mirror useless, for if it be not directed perfectly, still the rays will be returned on their own path, so long as the mirror is exactly in the focus of the object-glass.

3. There is very little loss of light if the instruments are in perfect focus. This was important to Fizeau, as it enabled him to use a long distance.

You can see the dimensions of the apparatus by inspection. The object-glasses are about $2\frac{1}{2}$ inches diameter, and 30 inches focal length. The disc is 4 inches diameter, and has 1,000 teeth. The distance employed by Fizeau was more than 5 miles; or exactly 8,633 metres. This was at Paris, from the height of Montmartre to a house at Suresnes. He found that the first eclipse occurred when a rotation of 12·6 turns a second was given to the disc. The

rate of rotation was measured by a counter which you may see attached here. We know then that light takes the same time to pass over 17,266 metres, as a tooth of the disc takes to reach the position of a notch. But to do this it must turn through the $\frac{1}{2000}$th of a revolution, therefore this interval of time is a second divided by the product of 12·6 and $\frac{2000}{25200}$. Hence, in one second light passes over a distance of 17,266 metres multiplied by 25,200. This is 435,000,000m. a second.

The final result arrived at by Fizeau was that light travels at the rate of 70,948 leagues in a second, there being 25 leagues to the degree.

The experimental investigation that has now been discussed is one of the most ingenious and difficult that have ever been undertaken. The chief difficulties arise from the want of light in the reflected image and the illumination of the field by extraneous light. The chief obstacles to an exact measurement by this means are, 1, the difficulty of measuring the velocity of rotation of the notched wheel at any moment. 2. The eclipse of the light is found practically to continue during a considerable variation of the velocity of the disc. Hence it becomes necessary to measure the velocity when the eclipse is first produced; and also the greater velocity when the eclipse ends. Hence it is necessary that the intensity of the source of light should remain constant. 3. It is a very difficult observation to determine exactly when an eclipse takes place. This depends very much on the amount of sensitiveness of the eye, which varies with the size of the pupil, a thing that is constantly changing. Seeing that there are so many difficulties in the method, it is to be regretted that Fizeau never published the details of his observations, so that we might judge of their agreement with each other.

We now come to a branch of the subject of enormous importance. I have already said that there were two theories to account for the propagation of light. The only evidence in favour of the undulatory theory, was that it explained a large number of facts that either could not be accounted for by the corpuscular theory, or else were explained in a clumsy way that was too artificial to merit belief.

This was a very important piece of work, but a great

deal was still left, which it was desirable to experiment upon. I have told you that the question of the velocity of light was able to satisfactorily settle the question between the corpuscular theory of light as enunciated by Newton, and the undulatory theory. How was that possible? It depended on the explanation of the theory of refraction. The refraction of a ray of light when it falls on the surface of a dense medium, is such that it is bent downwards towards the normal, as you know. In order to explain this, Newton supposed those particles of matter, the corpuscles, were attracted downwards towards a dense medium, and that therefore as soon as they came within the sphere of attraction of this medium, they came with greater velocity downwards, and so in passing through the medium they were deflected downwards, and also went with greater velocity. The explanation afforded by the undulatory theory, on the other hand, assumes that in a dense medium the velocity of light was less than in a light medium such as air, consequently it was pointed out, by Arago especially, that a determination of the relative velocity of light in air and water would be a conclusive crucial experiment to settle which of these two theories was true. At the time Arago pointed this out, the late lamented Mr. Wheatstone had just employed a most ingenious apparatus, the revolving mirror, in the determination of the velocity of electricity. Here we have a mirror belonging to the Paris Observatory, founded, I presume, simply on Wheatstone's model. It is a piece of mechanism designed with great skill by M. Breguet, the accomplished French mechanician, in order to give a great velocity of rotation to this little mirror. Wheatstone had used this mirror revolving with very great velocities. You can easily get a thousand revolutions in a second, and if you have such a velocity, and there is a beam of light falling upon it, then the reflected beam will be turned through an angular distance with very great velocity; and Arago proposed that a beam of light should be thrown on the revolving mirror, and then should be sent a little distance and reflected back again upon it. The mirror having been in that time rotated through a small angle, the reflected ray would be displaced through twice the angle through which the mirror was rotated. If the mirror

were fixed, the ray would be reflected back in a perfectly definite direction; but since the mirror is rotating, it will be turned through a sensible angle, and therefore it will reflect the ray back in a slightly different direction. This idea, proposed by M. Arago, was first successfully carried out by M. Foucault, and here we have the apparatus which was used by him in the determination of the velocity of light. This is the instrument he used to give a great velocity of rotation to the revolving mirror. This is simply a syren, an instrument for producing musical notes of different pitch, founded upon a principle first employed by Cagniard De La Tour. It is driven by air or by steam.

Foucault employed steam to drive this syren, and the steam or air plays a double part. In the first place, it causes, by an action similar to that of a turbine, the disc to revolve, and with it the associated mirror. In the second place, the air passing through these holes, which allow it to acquire very rapid motion from the air, produces a musical note, and the faster you rotate the mirror, the higher is the tone of the musical note produced. Here is one of a more powerful form, and you will be able to hear that the more rapidly the apparatus is blown the higher will be the note we produce. If we increase the pressure you will hear the musical note heighten in pitch. The tone of the note is gradually rising, as we increase the velocity of rotation. Then by means of a tuning-fork M. Foucault was able to tell the number of times this rotated in the course of a second, and the velocity varied from 200 to 800 revolutions. The light was sent from the side just as in M. Fizeau's apparatus. It fell on an inclined mirror, and was reflected through a lens on to a reflector, through that to another reflector, and then back again, so that by the time it got back, the mirror had turned through a sensible angle, and an observer here is able to measure the exact distance through which the beam of light has been deflected. M. Foucault employed here a network of fine platinum wires, eleven to the millimetre, which was illuminated by the light, and consequently when he examined it through this network, he was able to see the network, and he employed glass of such a thickness, that the reflection from the two surfaces of the glass overlapped; that is to say, that the first reflection of one of

these platinum wires exactly covered the second reflection from the other platinum wire, so that he got a double intensity of light. This was a very ingenious application of optical principles to increase the intensity of the light. In his communication to the Academy of Sciences in 1850, M. Foucault was simply measuring the relative velocities of light through air and through water, and he did so by interposing between two of the mirrors a tube half full of water, so that part of the ray of light went through the water and part went through the air, and he found that the light which was passing through the air was deflected from its natural position more than the light which passed through the water, in the ratio of four to three, showing that the velocity of light in air is $\frac{4}{3}$ of the velocity of light in water; exactly, in fact, in proportion to the refractive indices of the two media. This brilliant result completely settled the question of the two theories of light as they were at that time enunciated, and the undulatory theory has acquired, I will not say an absolute certainty, but we are much more nearly certain than we could possibly have been without such an experiment, and the corpuscular theory of light enunciated by Newton is absolutely untenable.

Exactly on the same day that Foucault communicated these brilliant researches to the Academy of Sciences, Fizeau also laid before the Academy a description of the same apparatus which he had mounted for the same purpose. But there had not been sufficient sunlight since it had been mounted in order to give him a definite result, and consequently he could not give the results at that time. In the year 1862 Foucault redetermined the absolute velocity of light in the same manner by employing the same apparatus which I have just described, and on this occasion he increased the distance which he employed, by interposing more mirrors before the light was finally reflected back again, so that he was able to get a distance five times as great as the one he had employed before; and in these experiments he employed the surprisingly small distance of nearly four metres at first, and in the second instance a distance of only twenty metres, and yet he was able to measure accurately that small interval of time which was taken by the light passing over that distance.

The results he communicated to the Academy in 1862 were stated by him to be remarkably accordant. He said that the experimental values he obtained did not differ by more than one hundredth part of the whole quantity to be measured, and finally, as the result of his work, he stated that the velocity of light was not what it had been supposed to be, but decidedly less; instead of being 308 million metres per second he found it was only 298 millions. He pointed out the very great importance of this result, because employing the constant aberration—that quantity which Bradley investigated, as it had been determined by astronomers, as being an angle of 20·45″, which was the value Struve gave to it—he found the parallax of the sun was no longer 8·57″, but 8·86″, and consequently by this measurement of the velocity of light he diminished the distance of the sun by 3,000,000 miles. This was a very important result, especially at the time at which it was announced. M. Hanson, the illustrious astronomer, whose tables of the moon are better than any constructed before, pointed out that the theory of the moon did not agree with observation, on the assumption that the distance of the sun is 95,000,000 of miles, and in order to make theory agree with observation it was necessary to reduce the distance between the sun and the earth to about 92,000,000 of miles. Now this was exactly what Foucault showed was necessary from the theory of the velocity of light. Other circumstances combined to render this true, and Mr. Stone, then Chief Assistant at the Royal Observatory at Greenwich, pointed out that when we discussed the observation of the transit of Venus of the last century, by interpreting the words of the observers in the true manner, those observations also did not give 95 millions of miles, as Encke had supposed, but gave a distance of somewhere about 92,000,000. All these facts combined together seem to make it nearly certain that that was the real distance of the sun from the earth. This was the reason that all the civilised nations of the world combined to get as accurate a value as they could from the transit of Venus which took place in 1874.

A great deal of importance attends this exact determination of the velocity of light, not only in this way, but also in other matters. In the system of observation which has

been brought into play during the last few years by the labours of Mr. Huggins by means of spectroscopic observations of stars, he is enabled to measure the velocity of the light of different stars towards or from the solar system, not at right angles to the line of sight, but in the line of sight; but those researches, the truth of which was considerably doubted by German astronomers until lately, have been confirmed by a magnificent new spectroscope lately erected in the Observatory at Greenwich. In order now to determine the velocity of approach or recession of different stars from or towards the earth, it is necessary to know with considerable exactness the velocity of light, and this is another reason why it is important to have this constant accurately determined.

Thirdly, and perhaps the most important of all, Professor Clerk Maxwell showed many years ago, in following out a brilliant idea of Faraday, that all electrical and magnetic phenomena are dependent upon the existence of a medium filling space between the electrified and magnetised bodies, and that there are lines of tension along the lines of force, and lines of compression perpendicular to the lines of force. Professor Maxwell, in following out this brilliant idea, and putting it into mathematical language, has been led to the conclusion that the function played by this medium imagined by Faraday is exactly such a function as could be played by the ether, of whose existence we have independent proof. And moreover there is a certain numerical quantity in electrical measurement which has been measured with great accuracy by Sir William Thomson and his assistant, a quantity which connects two systems of electrical measurements—the electro-statical system, and the electro-magnetic system of measurement. This quantity Professor Clerk Maxwell states, ought to be exactly the same as the velocity of light if the ether performs the function of that medium which Faraday conceived, and he has pointed out that this quantity, V, determined in electric measurement is as nearly as experiment will allow exactly the same as the velocity of light. This enormously important result of Clerk Maxwell's researches gives the determination of the velocity of light and of that constant in the electrical measurement of matter of the very utmost importance, because it is very likely to lead to a more

intimate knowledge of the nature of electrical and magnetic phenomena. Consequently different people have undertaken again to repeat the measurements of the velocity of light.

M. Cornu at Paris has repeated the experiment on Fizeau's plan, but he avoided this little source of error which I pointed out to you in making the determination of the velocity of rotation. He has an electric contact with one of these wheels, which marks upon a chronograph each revolution of the wheel. The chronograph is also in connection with a clock, which works a dot at each second; and by comparing the marks upon the strip of paper which are impressed upon it by this toothed wheel, and the marks impressed by the seconds pendulum of a clock, he is able to tell at any instant the exact velocity of rotation of the toothed wheel. He has also introduced probably other great improvements into the method of Fizeau, but the experiments have not been published in detail, so that we can hardly judge at present of their absolute accuracy and of their agreement with each other, but they have gone to show that the result obtained by Foucault was extremely close to what he found from his experiments by the method of Fizeau.

In conclusion, it may be worth while perhaps to point out a slight adaptation of the method of Fizeau which was undertaken by Mr. Young previous to the time when Cornu published his results. It has not as yet been completed, but at the same time it is perhaps worthy of being pointed out. The difficulty which is attempted to be got over is this: Fizeau had to observe the exact time when an eclipse took place of the distant spot of light—the exact velocity which was required to give the eclipse. Now you all know perfectly well that a very feeble light, when it becomes nearly eclipsed, will be so feeble that it will be invisible to the eye, so that there will be a sensible variation of velocity, during which the light will always seem to be eclipsed; and in order to get any accurate value at all M. Cornu in his experiment observed the velocity which first gave an eclipse and the velocity when he first saw a return of the light, and took the mean of those to indicate the true velocity, which gave the absolute eclipse. But it is very difficult to say when an eclipse actually takes place.

It depends very much on the condition of the eye and the dilatation of the pupil. All experiments go to show that it is very difficult to say exactly when a feeble light is visible and when it is not visible. Now the method employed by Mr. Young avoids this completely. Instead of having one collimator at the distant station, there are two, one fixed about half a mile further away from the observer than the other. As we increase the velocity of rotation of the toothed wheel, the reflection from the more distant collimator will of course be the first eclipsed. At first two lights are seen on the edge of the wheel, namely, the two lights reflected from the two collimators. As you increase the velocity of rotation you first see the light reflected from the distant collimator eclipsed, while the other one is still tolerably bright. You increase the velocity, and the distant collimator comes again into view before the second one has been eclipsed, and you compare the intensity of those two lights, and may measure the velocity which is necessary in order to render those two lights exactly equal, the one having passed its eclipse and the other not yet having reached its eclipse. When you have got that exact velocity, and have found the two lights exactly equal, you know that is the velocity required to produce an eclipse, if you had one collimator fixed half-way between the two. This method will probably give very much greater accuracy than any other which has been employed, but no absolute measurements have yet been obtained.

There is only one step further which we can hope to go in these measurements of the velocity of light. There is one point which has been always necessary in the undulatory theory of light, and upon which experiments are wanted to render it still more certain, and that is the question of dispersion. This was an objection which was raised against the undulatory theory long ago. Granting these experiments and the results to which they lead, the velocity of light in water and in air ought to have a definite ratio, because the elasticity of the ether in water has a definite ratio to the elasticity of the ether in the free air; consequently the elasticity of light in water of all colours should be the same; but we know perfectly well from experiment that a beam of white light falling on the surface of a dense medium is divided into its different colours, the dark rays

being bent down much more than the red rays are, and consequently this undulatory theory must assume that the velocity of light is different for different colours in a dense medium. We know that it is not so in ether or in the atmosphere to any appreciable extent, for otherwise, in the occultation of a star, when a star reappears from behind the surface of the moon, if the violet rays travel quickest, the star should first appear violet, and then the other colours should gradually be added; but such an effect has never been observed. But it is quite possible that in a dense medium the velocity of the violet rays may be less than the velocity of the red rays. The theory has only partially been worked out by the brilliant investigations of M. Cochet, but he founded his theory on an assumption as to the nature of the ether which is quite untenable; nevertheless the nature of his results was to show that the velocity of light in a dense medium would depend on the wave-length of light, that is to say, on its colour; and it is extremely probable that although M. Cochet employed an erroneous theory of the nature of the ether, that any true theory which we could work out would lead to the same results as those which he got. However, it is a very great desideratum to prove that the velocity of light in a dense medium is dependent on the colour of the light which is passing through the medium, and it is to be hoped that before many years such a result may be obtained.

I think I have now described nearly all the facts which have been observed in connection with the velocity of light hitherto, and you will see that although it is a subject of very great difficulty, it has been attacked from very different sides, and in all cases very concordant results have been obtained; and this does enormous credit to the ingenuity of the methods employed, chiefly because these methods were so entirely original.

APPARATUS FOR PHYSIOLOGICAL INVESTIGATION.

BY DR. BURDON-SANDERSON.

WHENEVER it happens that I have to discuss the progress of discovery in physiological science during the present century, the fact forces itself on my mind, that the year 1850 marked the commencement of a new era—an era which can be compared in importance with none excepting that of Harvey. The two epochs differed, however, from each other in one remarkable respect: that, whereas Harvey's achievement was a consummation, the other was merely a commencement. Harvey, by his crowning discovery, gave meaning and completeness to the discoveries of his predecessors, but the process which began from the middle year of the present century has been a gradual one, of which the rapidity and extent are still increasing. It cannot be regarded as the work of any one individual, or as the result of any one discovery. If it be asked how this is to be accounted for, the answer must be, that it arose out of the still more rapid progress already made in those more exact branches of natural science included in the terms physics and chemistry, on which physiology is based, and of the application of their methods to the investigation of the phenomena of living beings. I will give you three instances of this: the application of the microscope to the investigation of the structure of plants

and animals; the application of instruments of measurement to the investigation of the electrical and other changes which present themselves among the physiological effects of excitation of nerves and muscles; and, *finally, the application of recording instruments to the investigation of the movements of living beings.* Each of these *applications* has great names associated with it—the names of the men, all of them now living, who, some thirty years ago, began the great work of building up physiology on an entirely new foundation. Among the microscopists may be named Henle, Kölliker, Bowman; of vital physicists, Helmholtz, Donders, Brücke, Du Bois Reymond, and last, but not least, Ludwig.

It was by Ludwig that the method of investigation, of which I have to give you some illustrations to-day, was introduced. He invented and constructed the first registration apparatus somewhere about 1846. Before I describe it to you, I must explain the meaning of the term.

Let me then first state that registration, in physiology, means the obtaining of a written line or tracing which shall exhibit the position of a moving body at every moment of time during the period of observation. The method by which this is effected is called the graphic method.

It follows from this definition that the motion to be recorded must be a linear motion, *i.e.*, a motion along a given line. I can best explain this by proceeding at once to examples.

Suppose that I wish to inscribe graphically on this sheet of paper the motion of a train travelling between South Kensington and Queen's Road,[1] how would I attempt to do it? I make my sheet of paper rectangular, for I have two processes to record simultaneously, which, for the moment, I conceive of as going on in directions at right angles to each other, one of which, viz., time, is uniform, the other, viz., the motion of the train, is variable. My purpose is to compare the variable with the uniform, *i.e.*, to record motion in relation to time.

I begin by marking off against the side of my paper the whole distance in miles from one end of the line to the

[1] Names of stations on the Metropolitan Railway.

other—say two miles; then I divide that distance into quarters. Then, knowing that the train requires twenty

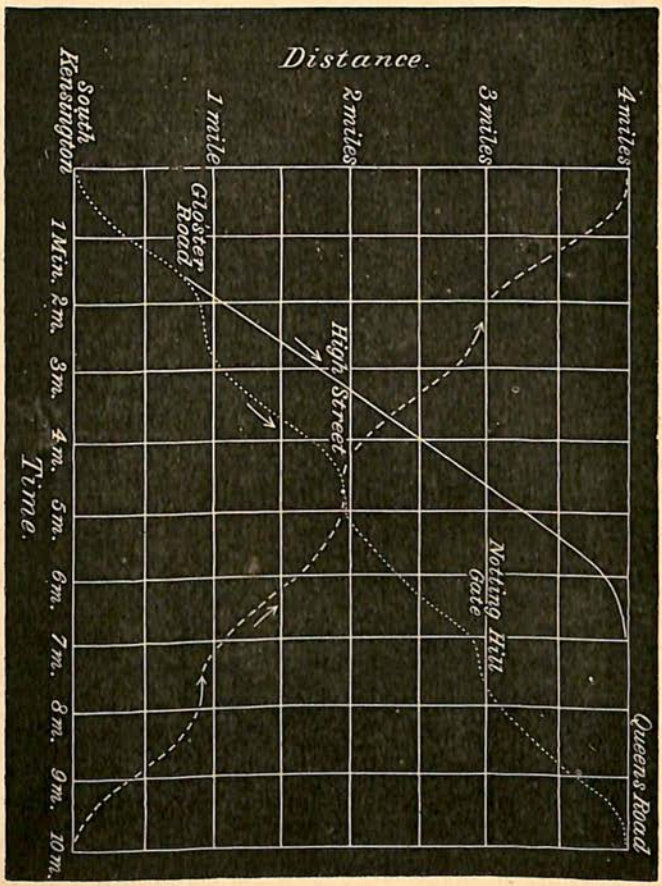


FIG. 1.—Diagram showing courses of trains from South Kensington Station to Queen's Road and *vice versâ*. The dotted line exhibits the progress of a train running towards Queen's Road and stopping at each station; the broken line shows the progress of a similar train in the opposite direction; the continuous line that of a train running the whole distance without stopping.

minutes to accomplish this distance, I measure off along the horizontal edges twenty divisions. I divide it first into

four equal parts, and then each of these into five, corresponding to minutes. I then connect the corresponding points at the opposite edges, and so divide my field into rectangular spaces.

Now on the space so divided it is obvious that the position of any body (say a train) travelling along a definite course from South Kensington to Bayswater in twenty minutes or less can be readily shown. To do this I have to set off on the vertical or ordinate corresponding to each minute the actual position of the train at the end of such minute, according to the distance it has travelled from its starting-point. Thus, the train has arrived at Gloucester Road in four minutes; I therefore indicate its position at the end of four minutes by measuring off three-quarters of a mile, viz., the actual distance between South Kensington and Gloucester Road, along the fourth ordinate. This done, I connect the point so found with the starting-point. If the train has in its journey from South Kensington moved at a uniform rate, then the line is a straight one. If, as would certainly be the case, it started slowly and slowly stopped, that would be marked thus:—(See Fig. 1.)

It is obvious that on the same plan the motions of any number of trains, whether going in the same or in opposite directions, could be represented. In fact I have been told that a graphic method such as I have been describing has been employed in the construction of the time-tables of some of the French railways.

Let me pass from this example to one in which the graphic method is used to register a fundamental physical fact or law, *e.g.*, the law of falling bodies—an example which I choose because it was the first motion to which the graphic method was applied in physics. You have before you a cylinder (Fig. 2) which revolves on a vertical axis at a uniform rate, viz., forty times in a minute. Here is the falling body, which, in order to accomplish the result we want, we have suspended by a thread in such a position that when it falls its motion is recorded on the blackened surface of the cylinder. The clockwork having been set in motion, I will now apply a lighted match to the thread. At the moment that it is burnt through, the weight will fall, and its motion will be recorded on the surface of the blackened cylinder. It will describe on that surface a line which will resemble

that I have drawn on the blackboard—a line which I doubt not is perfectly familiar to you. That line expresses with very great accuracy the *law* of falling bodies; for its curvature is such that it could only have been produced by a body moving with ever-increasing velocity.

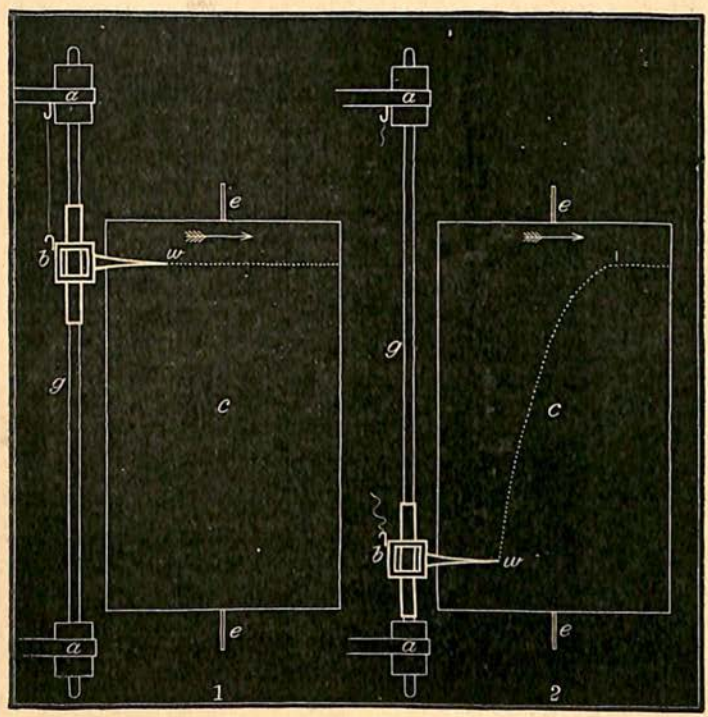


FIG. 2.—Extemporised apparatus for investigating the course of a falling body in its descent. *a a* in each figure are two corks held by clamps, by which a glass rod, *g*, is supported vertically at a short distance from the surface of the revolving cylinder *c*. The falling body *b* (a block of brass pierced by a tube of the same metal) slides freely on the rod. At its upper end a thread is attached by which it is prevented from falling until the proper moment. The cylinder having been put in rapid rotation, a match is applied to the thread, *b* falls and writes by the style, *w*, the line shown in the right-hand figure.

I will give no further illustration derived from physics, for my purpose is to show you the application of the method to vital motion. The characteristic phenomena

which are displayed by a living animal as such—its functions as they are called—may be divided like those of the inorganic world, into chemical and mechanical.

As instances of mechanical functions I may mention locomotion, respiration, circulation. All of these consist in the performance of motions which can be registered by methods similar to those we have used for registering the movement of a falling body. All of them are performed by the combined action of various forms of muscular apparatus.

The vital motions first registered were those of the circulation. The first graphic apparatus ever used in physiology was, as I have already mentioned, Ludwig's, and the purpose for which he devised it was the measuring and recording the variations of hydrostatic pressure in the arterial system. To enable us to understand this, I must place before you one or two physiological facts.

I will assume that you are acquainted with the general arrangement of the circulatory system, *i.e.*, that it consists of the heart—a pump, and various tubes for the conveyance of blood, namely, arteries, capillaries, and veins. You probably also know that the arteries act as a distended reservoir, *out of* which in the forwards direction blood is constantly flowing, *into* which from behind blood is being pumped at intervals by the heart. Now for all the practical purposes of life what is essential is that blood should flow through the capillaries of every part of our body in a constant stream. What is the cause of this motion? The obvious answer is, Because the heart pumps the blood on. This is a true but a very imperfect answer. Its incompleteness consists in this—that instead of giving the immediate cause of the phenomenon, you give the more remote one. The immediate cause of the circulation of blood through the capillaries of my finger is not the pumping action of the heart—obviously not; for if I could put the capillaries of my finger under the microscope as I can the web of the frog's foot, I should see that the stream does not keep time with the stroke of the pump, but goes on in a continuous and equable flow.

In the transparent web of the foot of the frog I can actually see that this is so, for if while I am watching the flow of blood through the capillaries of the web, I suddenly

arrest the action of the heart, I find that the capillary blood-stream continues for several seconds, notwithstanding that no more blood is pumped along the arteries, and thereby learn that the circulation through the living tissues is only *mediately* dependent on the action of the heart.

What then is the immediate cause of the motion of blood in the capillaries? The cause is simply the difference of internal pressure between the arteries and veins. By this, I mean that if it were possible to connect with one of my arteries (say the radial) a pressure gauge by which I could determine the difference between the pressure exerted by the liquid on its internal surface and that of the atmosphere, and to connect a similar gauge with one of the veins by which blood is brought back again from my hand, I should find that there was a difference between the two gauges of about four inches of mercury—in other words, that in the arteries the pressure would exceed that in the veins by something like two pounds on the square inch. That is the simple reason why the blood flows out of the radial artery into the veins which come back from my hand.

The first person to demonstrate this fundamental fact in physiology was a beneficent clergyman of the last century, Dr. Hales, who made experiments, in which, having inserted a long vertical tube into an artery of a living animal, the other end of which was open, he found that the tube filled to a height of several feet, the column of blood dancing up and down at each stroke of the heart pump, whereas, when he inserted a similar tube into a vein, the blood either did not rise at all, or rose only a few inches.

For a long time physiologists were content to leave these facts just as Dr. Hales first discovered them. It was not until late in the present century that Poiseuille gave them precision by substituting for Dr. Hales' tubes the mercurial manometer, *i.e.*, a glass tube bent into the form of a U, supported vertically on a suitable stand, and half filled with mercury. If one limb of such a tube is in communication with an artery, in such a way that the same pressure is exercised on the surface of the mercury contained in it as on the internal surface of the artery, while the surface of the column of mercury in the other limb remains open, the difference between the pressure in the

artery and that of the atmosphere may of course be judged of by the difference between the heights of the two columns. The instrument Poiseuille used was called a Hæmadynamometer, because it was supposed to measure the force of the circulation. By means of it he discovered that the arterial pressure varied in different creatures, partly according to the size, but that it was tolerably constant in the same animal when in health; and he inferred, from measurements made on the higher animals, that in man it amounts to about eight inches of mercury.

This discovery was one of practical as well as theoretical value, for the maintenance of good pressure in the arteries is as essential to healthy life as the maintenance of a proper temperature. There is no acute disease in which our arterial pressure is not disturbed, scarcely a remedy or poison of which the action does not more or less depend on the effect which it exercises on the state of distension of the blood vessels.

It was for the investigation of the variations of arterial pressure that the graphic method was first applied by Ludwig. The instrument first used by him for the purpose differs from that of Poiseuille, only in this respect—that a contrivance is added to it by which it is converted into a graphical instrument. On the surface of the mercury column in the open limb of the U tube a light float, made of black caoutchouc, rests. To this float is connected a stem of the same material which near its upper end is crossed by a horizontal pencil (Fig. 3). I will now communicate an oscillatory movement to the mercury column on which the float rests, by working a syringe, of which the interior is in communication with the other limb of the U tube. You will see that every motion of the mercury column will be followed by the float, and accurately recorded on the cylinder by the pencil.

Here is another manometer constructed on the same principle, which I have put on the table simply for the reason that it was the first ever made in this country. The stem of the float, instead of having a pencil attached to it directly, supports a long, carefully-counterpoised lever, by which, in a manner that I shall explain more fully afterwards, its motions are amplified. I exhibit to you tracings of the variations of arterial pressure made with this machine.

FIG. 3.—Recording manometer. The shorter limb of the bent glass tube *c* communicates at *a* with the cavity in which the variations of pressure are to be recorded by a flexible but inelastic tube. The tube *b* is for filling the instrument; it is closed by a screw clamp. The mercurial column in the open limb is represented as 13 centims. higher than in the other. On the surface of the higher column rests the float *f*, from which the rod *e*, with its writer *w*, rises; the rod moves easily in the support *d*.

The word kymograph,[1] which is applied to this and all

[1] The word is incorrectly constructed, but has been so long in use that it would be difficult to replace it by a better.

other instruments of the same kind, is derived from the Greek word κῦμα, which signifies a wave. It is so called because it is used to record the wave-like variations of arterial pressure, just as other instruments, of which there are beautiful examples in this collection, are used by the physicist to record the variations of the surface of the ocean. Just as in the ocean two principal orders of variation have to be recorded, *viz.*, tides, and waves in the ordinary sense, so here there are slow variations of considerable duration which recur at relatively long intervals, and others which are by comparison momentary, but of great frequency. Of the former, the mercurial column gives a direct translation, so that the record of its ascents and descents is equivalent to a direct record of the expansions and contractions of the artery itself. But as regards the more frequent ones it is not so, for this reason, that any sudden impulse imparted to the column of mercury in a manometer gives rise to a series of consecutive oscillations, of which the duration and character have nothing to do with that of the motion originally imparted, but depend on the form of the instrument. They are therefore called instrumental.

This being the case, the mercurial kymograph has only a limited application, as an instrument for recording the actual movements of an artery. It is perfectly adapted for the purpose of measuring and recording the *mean arterial pressure*, and such variations as are of relatively *slow* progress, but it is incapable of translating to us those *rapid changes* which occur in the arterial pressure between one stroke of the heart and another—changes of which we estimate the duration not in minutes, but in fractions of seconds.

That this defect is an important one is patent from the following consideration:—Every one is aware that the quality of the pulse is matter of great practical interest. Now, just as the physicist knows precisely how to account for the difference of quality of different musical sounds, by their physical characters, and is able to show that each *timbre* may be represented graphically by a curve of which the characters can be determined beforehand with mathematical accuracy, so that, by the use of methods of measurement, he acquires a power of discriminating

which rivals that of the accomplished musician, so the physiologist is able to assign to each of the fine varieties of the human pulse, which are appreciable to the accomplished touch of the physician, a curve of definite form—a curve expressive of that rapid succession of movements to which the pulse owes its quality.

The quality of the pulse has been long appreciated in medicine, and the *tactus eruditus* has been long spoken of as the faculty of the accomplished physician, who by feeling the pulse with his finger can not only count its rate, but also judge of its quality, for describing which he employs certain more or less conventional terms, such as hardness, fulness, sharpness, and others, all of which are of practical significance. There is therefore a practical reason why it is necessary not merely to study those slower variations of arterial pressure which I have compared to tides, but also those finer and more rapidly recurring changes on which the peculiarities of the human pulse depend. Now in order to illustrate this, I desire to make some experiments; and as we cannot make them on a living artery, we shall use what is called in physiology a schema, *i.e.*, a mechanical apparatus so constructed as to imitate the living organ in respect of some of its properties.

Our schema consists of an elastic tube resembling, as regards the thickness and elasticity of its walls, an artery. At one end it is in communication with a pump, at the other with a tube of outflow, of which the diameter can be regulated by a screw. Such a tube represents an artery in respect of several of its properties.

One of the most important is that it converts the intermittent motion with which water is injected into the tube at the end next the pump into an equable or constant progressive motion at the other. You see that the rate of outflow is nearly constant, but not quite so, for if I allow the water to flow out horizontally, the curve it describes in falling is one of varying declivity. At each stroke of the heart-pump it is thrown a little further out.

To understand this equalization all we have to do is to consider what happens to the motion communicated to the liquid at each stroke. At the moment that the impulse is given, liquid enters the tube at a rate which exceeds that of the efflux. Motion consequently has disappeared.

What has become of it? It has been lost in distending the elastic wall of the tube. Let us next consider what is the state of things immediately after the impulse, *i.e.* between one stroke of the heart-pump and the next. At that moment the rate of efflux *exceeds* that of influx. Hence motion has reappeared. Where has it come from? It has come from the distended elastic wall of the tube, which is now *repaying* to its contents that which immediately before it borrowed. So long as the pump continues to work, this process, by which during the stroke motion is lent by the contents to the wall, and repaid by the wall to the contents during the interval, goes on.

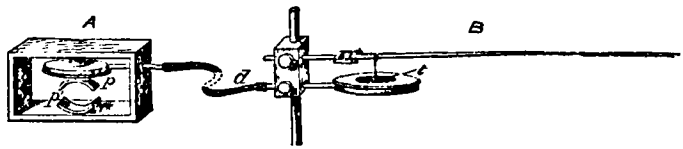


FIG. 4.—Instrument for recording the progress of a wave of distension along an elastic tube containing liquid. A may be described as a box two inches long and one inch high, without sides; it contains the receiving tympanum. B is the recording tympanum and its writing lever. The tympanum *t* is a metal box without a top, of such a size and shape as to contain a florin piece. Over this box a sheet of indiarubber is extended and fixed air-tight. To this membrane a thin metal plate adheres, which is connected by a jointed vertical rod with the long lever. The joint is very near the axis on which the lever moves. The horizontal rod by which the tympanum is supported is tubular and communicates with the cavity of the tympanum. To its end is adapted a flexible tube *d*, by which the two tympana communicate, and which may be several feet in length. The receiving tympanum is of the same form and construction as the other, but is upside down, its bottom being screwed to the under surface of the top of the box. If an elastic tube, along which liquid is being driven in an intermittent stream by a pump, is placed between the two brass pieces *p p'* shown in the figure, the increase of diameter of the tube which occurs at each pulse produces a vertical motion upwards of the membrane of the receiving tympanum, and a corresponding movement in the same direction of the other. This last motion is written on the blackened cylinder by the style at the end of the lever. For investigating the course of a distension wave along a tube at least three pairs of tympana are required. In using them the recording tympana must be placed on the same vertical rod, one above another.

This process of alternate distension and relaxation is betrayed to us by visible motions, and my next object will be to show you how we may obtain a graphical record of these motions, and to explain to you their characters. Let us first fix our attention on the instantaneous distension which is produced at the near end of the tube, *i.e.* at the end next the pump, at each stroke, in consequence of the injection of a certain quantity of liquid. If we arrange

our apparatus (Fig. 4) in such a way that we can obtain a continuous record of the changes of diameter of the tube which are taking place simultaneously at both ends and in the middle, and compare the course of events at the near end with what is happening during the same period at the far end, we shall be able to see that the distension effect gradually progresses from the former towards the latter. For this purpose two little instruments, called tympana, have been connected—the one with the proximal end of the supposed artery, the other with the distal end. Half way between the two there is a third tympanum. Each of these is connected in a manner which I shall explain presently, with a second writing tympanum, by which its motion is registered. We thus get a triple tracing; one relating to the initial part of the artery, one to the middle, and the other to the end part of it. [Here the experiment was made.] It is not possible for you to see the record which the three levers are tracing on the cylinder while Mr. Page works the pump. I have therefore represented their characters in this diagram (Fig. 5). These black lines on the diagram represent the three simultaneous tracings made with the apparatus at different distances from the initial point. The lower one is the graphic representation of the distension wave, or rather waves, produced by a single injection of liquid close to the pump; the second is the tracing obtained when the tympanum is applied to the tube about half-way between the starting-point and the end; the top line relates to the part of the tube close to its extremity, where the water is continuously discharged by an orifice considerably narrower than the tube itself. Let me now draw your attention to the correlation of the three tracings. In the lowest line you have the record of a first distension, which is followed after a short interval of time by another. In the second line the same thing, with this difference—that the interval between the primary wave and its repetition is only half as long. The third line exhibits the fact that at the end of the tube the wave is not double but single. The meaning of these differences is easy to understand. Just as when you make a series of waves by throwing a stone into the centre of a pond, the wave gradually progresses towards the side, and then returns upon itself towards the centre; so here in the case

of the elastic tube, the distension progresses from its starting point until it gets to the further end, and from thence it returns again. In the one case as in the other, the difference of time between the first wave and its repetition depends, as you will readily understand, on the distance from the terminal point of the system to the point at which the observation is made.[1]

Having got thus far—having before us an actual case, a motion of the *kind* which presents itself in the living organism, an apparatus by which I have translated that motion to paper—I will proceed by explaining to you the principles of its construction, and how they may be applied to the investigation of other vital movements. The apparatus

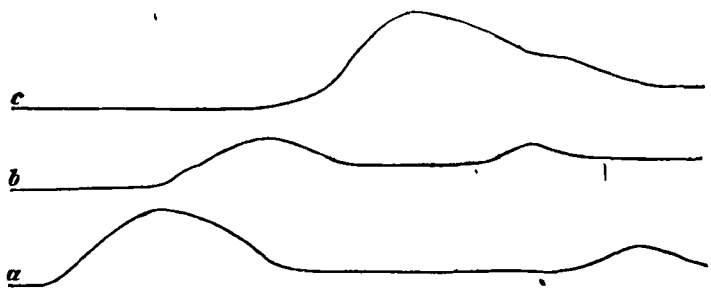


FIG. 5.—Graphical representation of the characters of a distension wave in its propagation from the near to the far end of a closed elastic tube and back again. *a*, near commencement of tube; *b*, near the middle; *c*, near the end.

illustrates (1) the mode of immediate transmission commonly used, and (2) one of the most important modes of indirect or mediate transmission. In the apparatus you have had before you, the motion of the tube which represents the artery is communicated first to an indiarubber membrane. (See Fig. 4.) Then the motions of that membrane are written on the paper. Let me first explain how this more simple result is attained. The simplest of all arrangements to magnify and inscribe a linear motion of small extent consists of a lever of the second order, of which one end is pierced by a *fixed* axis, on which it can rotate freely,

[1] It is proper to note that in the schema described, the motions of the arterial wall are only partly represented. In the arterial system there is no terminal point or obstacle by which the wave is reflected.

while at some other part of its length it is supported horizontally by the moving body.

This principle is applied in a great many instruments, of which the best known is Marey's sphygmograph for obtaining records of the human pulse. In order that we may see the thing in its simplicity, Mr. Page will make an experiment on his own pulse. Having placed a stiff band of pasteboard across the right wrist, and secured it in its position by a strong indiarubber band, he takes a long light lath in the other hand, and insinuates one end of it, which is shaped to a chisel edge for the purpose, under the lower edge of the slip of pasteboard, the opposite end of the lath being extended in the direction of the arm, over the palm of the hand. This done, the cardboard serves as a

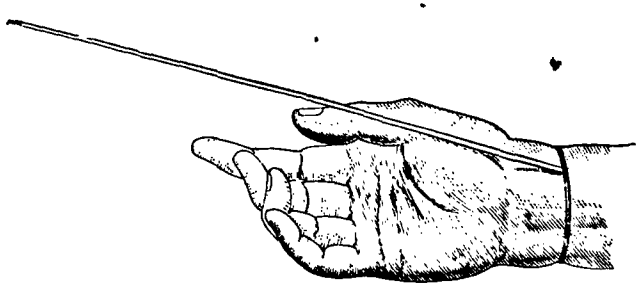


FIG. 6.—Simple mode of obtaining a graphic record of the radial pulse.

fixed axis on which the lath can rotate, and if it is so placed that it rests on the pulsating artery at a very short distance from the card, the extremely inconsiderable movement, which is actually communicated to it by the expanding artery, shows itself at the opposite end of the lath as an oscillation of sufficient amplitude to be seen at any distance. In this way, and by means of this very simple apparatus, we may obtain a tracing which exactly corresponds to the tracing which you will see immediately obtained with the sphygmograph. Let us look first at the sphygmograph itself, of which, however, I can only give you a very short description. It consists of a framework of brass, which is capable of being fixed to the arm in such a way that it is, so to speak, in one piece with the bones of

the wrist and forearm. Attached to the frame is a spring, which rests on and presses the artery. This being the case, the spring follows exactly the movements of the artery. The rest of the instrument consists essentially of a long lever by which the motion of the spring—*i.e.* of the artery —is written on a plate which is made to travel at a uniform rate by watch-work.

This is one application of the direct method. There are several other examples which might be taken, but I must hasten on to describe to you the method of mediate transmission.

Here is an apparatus of M. Marey's which is intended to illustrate this. It consists of two tympana, constructed as I have been describing to you, and in communication with each other by a flexible tube of several yards in length; notwithstanding this, any motion which I communicate to the near, or, as we may call it, the receiving tympanum, is as perfectly transmitted to the distant or recording tympanum as if the two membranes were stretched over the end of one cylinder.

It is obvious to you that the value of this contrivance, as a means of investigating physiological motions, depends entirely on its accuracy. It is therefore of fundamental importance to be certain that the motions communicated are faithfully imitated.

In the beautiful collection of instruments exhibited by Prof. Donders there is one (No. 3,955) which is specially designed for this purpose. It was designated in the first edition of the Catalogue by the rather quaint name of "Controller of the Air Conveyance." It consists of three essential parts, namely—first, a cam,[1] by the rotation of which a very complicated series of up-and-down motions are communicated to any body capable of such motions, which reposes on its upper edge; secondly, a lever for direct transmission of these motions to a recording surface, of the same kind as those already described to you; and thirdly, a couple of tympana of which the cavities are in

[1] The cam used in this experiment was of such form that the successive variations of radius in each revolution corresponded very closely to those of the transverse diameter of the heart of an animal; so that the curves or tracings recorded closely resembled those which are obtained when the motions of that organ are recorded by the cardiograph.

communication by a long flexible tube. You observe that the lever of the recording tympanum and the lever on which the cam acts directly, are so arranged that they move in the same vertical plane, that their writing points are directly one above the other, and that both points are in contact with the surface of a blackened sheet of paper with which this cylinder is covered. By this arrangement two records are obtained simultaneously of the same motion —one by immediate transmission by the lower lever, the other mediately by the two tympana. If you compare them you will be unable to detect any difference between them. In fact, for practical purposes they are identical: there are, however, certain slight differences which have been investigated by Prof. Donders.

I will now proceed to describe to you various applications of this method to the investigation of physiological motions, and will first refer to the remarkable researches of M. Marey on locomotion. The principal purpose of these researches was to determine with much greater precision than was before possible the time relations between the various motions which are executed by the body and limbs in different kinds of locomotion, as, for example, in walking, running, or flying. I can best illustrate this by asking you to look at this large diagram, in which a man is represented running and carrying with him a recording apparatus, consisting of clockwork and tympana, by which his motions are faithfully transmitted to paper. You will observe that there are three pairs of tympana, viz., three recording ones which inscribe their records one above the other on the small revolving cylinder which he carries in his hand, and three contrivances which correspond in function to receiving tympana. Of these two are for recording the time during which, and the degree in which, the ground is pressed upon by each foot in the act of running. You will readily understand this if I show you one of the shoes which the runner wears. In the sole, which is made of vulcanised indiarubber, there is a flattened cavity, which communicates with that of the recording tympanum. You will readily understand that the air it contains is compressed by the foot, just so long as the weight of the body rests on it. The third receiving apparatus rests on the top of the head and communicates to the tympanum, with which it is connected, the up and

down motions of the body. All three recording levers work in the same plane. Their points are directly one above another, and they write on the same cylinder: here is the record obtained. We can see at a glance that in running, the body is entirely unsupported during a short interval between each step and its successor, and we can, if we like, measure the duration of that interval. We can also determine at what moment during the period that the foot rests on the ground the pressure is greatest, and what is the effect of that pressure in raising the body. I regret that there is not time to go further in describing M. Marey's beautiful contrivances for carrying out his other researches in the same field; particularly those most interesting ones which relate to the mechanism of the flight of birds. I must content myself with stating that by training pigeons to carry suitably contrived recording apparatus he has obtained graphic records of their motions of flight, which are quite as perfect as those of which I have given you examples.

I will now show you one or two contrivances for recording the motions of the chest in breathing. Here is a very simple one of M. Marey. It is an inelastic cincture, the length of which can be varied so as to adapt it to the girth of the chest. Each end of the band is connected with a receiving tympanum, which is expanded when the chest enlarges, and *vice versâ*. In this way the variations of girth of the chest can be recorded, with a certain degree of accuracy.

The other instrument (Fig. 7) is perhaps not quite so easily applied, but gives results of much more value. Its purpose is to measure not the variations of girth but those of diameter of the chest; and it affords a good illustration of the possibility of obtaining results of great accuracy by this method. In ordinary breathing the variations of the transverse diameters of the chest, whether measured from side to side, or from before backwards, are very inconsiderable, that is to say, the *increase* of diameter which takes place in inhaling air is very small indeed, as compared with the total diameter of the expanding chest. Thus in tranquil breathing, the chest of a full-grown man becomes wider only by a line, whereas its total width may be from ten inches to a foot. The measurement of so small an increment

is a matter of some nicety, but of much practical importance, for by measuring in succession different diameters, we can determine the enlargement of volume, with much more accuracy than by any other method that is applicable to the human body. The apparatus consists of a frame, of which the shape resembles that of the Greek letter Π. One

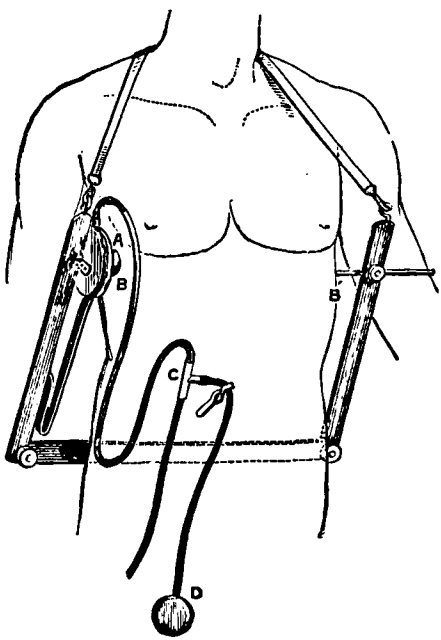


FIG. 7.—A, tympanum ; B, ivory knob ; B′, rod which carries the knob opposed to B ; C, T tube by which A communicates on the one hand with the recording tympanum, on the other with an elastic bag D. The purpose of the bag is to enable the observer to vary the quantity of air in the cavity of the tympana at will ; the tube leading to it is closed by a clip when the instrument is in use.

end of the Π rests, in case it is the antero-posterior diameter that is to be measured, by an ivory knob against a dorsal spine, while between the other and the anterior surface of the chest a receiving tympanum is interposed, by which the motions of the sternum are transmitted to the recording tympanum. A tracing is obtained in which the original motion is represented, amplified about twenty times.

Here is another instrument, constructed on the same principle as the others, the cardiograph—its purpose being to record those motions of the wall of the chest, which are occasioned by what is called the *impulse* of the heart. Each time that the heart contracts, it alters its shape, becoming globular and much harder than it was immediately before. The consequence of these facts is, that the moment of contraction is marked by an expansion of that part of the wall of the chest nearest the heart—an expansion which we

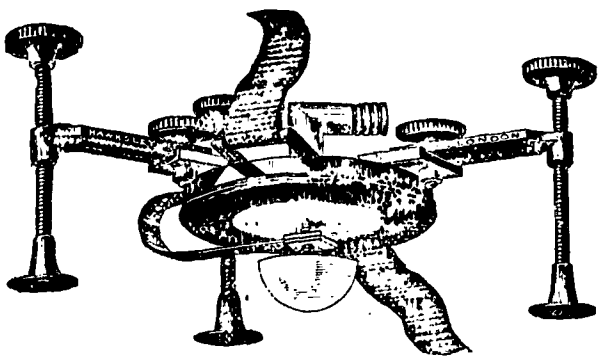


FIG. 8.—The Cardiograph. The tympanum rests on three legs, of which the lengths may be varied. The ivory button is supported by a steel spring, so that the movements imparted to it by the wall of the chest are communicated to the membrane of the tympanum, the surface of which faces downwards. The cavity of the tympanum communicates with a second (recording) tympanum, by a flexible tube, not shown in the drawing. The instrument is secured in its position by a strap, which is buckled round the chest.

can all readily feel when we place the hand on the left side immediately below the nipple. This motion is different in different persons, and under different conditions of health. An importance therefore attaches to it which is similar to that of the better known phenomenon which we have already studied,—the pulse. I show you the tracings obtained by it, but I will not dwell on their characters, for their significance can hardly be appreciated by any one who is not already familiar with the structure and mode of action of the heart. It will be sufficient to say that by the cardiograph we can measure not merely the rate, *i.e.*, the frequency of the contractions, and appreciate their vigour,

but can determine the intervals of time which elapse between the opening and closing of the two sets of valves, on the due working of which, the efficiency of the organ as a pumping mechanism very principally depends.

I now come to my last illustration, and I have put it last, because I attach more importance to it, than to the others; for it involves a physiological experiment of fundamental importance. I have prepared a muscle, the calf-muscle of a frog, which was killed immediately before the lecture. The muscle, although the frog of which a while ago it formed part is dead, is itself living, *i.e.*, it still enjoys the same power of contracting, though in diminished completeness, that it possessed when still part of a living organism. Attached to the muscle is its motor nerve, which has also been carefully prepared. Probably most of those present are aware that when the motor nerve leading to a muscle is interfered with so as to become excited or irritated, the muscle contracts. If the excitation is of an instantaneous nature, such as is produced in this instance by the passage of a single induction shock from this induction machine, a single and apparently instantaneous shortening of the muscle takes place, which if the tendon of the muscle were still attached to the foot, would suddenly extend it. As you will be able to see after the lecture more distinctly, the end of the muscle nearest the knee joint is immovably fixed to a block of cork; the tendon (*i.e.*, the Tendo Achilles) is attached by a silk cord to a lever so arranged, that when it is acted upon by the contracting muscle, the receiving tympanum *c*, is pressed upon. By the mechanism with which you are now familiar, the motion thus communicated is transmitted to the recording tympanum *d*, and written by it on the blackened surface of the revolving cylinder.

The cylinder revolves forty times in a minute. Its circumference is twenty inches, consequently the rate of horizontal motion of the surface of the paper is 800 inches per minute or 13·3 inches per second. In connection with the cylinder there is an arrangement, by which, each time that a certain point in its circumference touches a trigger, the primary circuit of the induction apparatus is closed, so that a so-called "closing" shock is sent through the nerve, by means of a pair of platinum electrodes which are in contact

with it.[1] Let me point out to you further that the recording tympanum is supported by a vertical rod which is of one piece with the clock-work and cylinder, so that its distance from the axis of the cylinder is invariable—and that on this rod there is a rack and pinion, by which the recording tympanum can be fixed at any desired height.

I will now perform a series of experiments before you, each of which will consist in allowing a single induction shock to pass through the nerve, and recording the resulting muscular motion on the cylinder. And in order that the result of each experiment may be distinguishable from that of its predecessor, and at the same time readily comparable with it, Mr. Page will by means of the pinion I have mentioned, so adjust the recording tympanum that each tracing shall be a twenty-fifth of an inch below the one last made.

We have now made ten records. I will next bring the catch *h* on the cylinder into contact with the trigger, which closes the primary circuit of the induction apparatus, so that the cylinder shall remain for a few moments in the exact position, which, when revolving, it attains at the moment that the induction shock passes through the nerve. The muscle contracts and compresses the receiving tympanum. The consequent expansion of the recording tympanum elevates the lever attached to it, and thereby makes a vertical mark on the recording surface. From this vertical mark I draw with the aid of the rack and pinion a *vertical line*, which of course cuts all the ten tracings at right angles. My object in doing so, is that the intersection may mark in each tracing, that point in it which corresponds to the instant at which the nerve is excited.

Let us now study the results. On the left side of the vertical line, the record consists of ten horizontal lines, one below the other at a distance of one millimetre each from each. Beyond, *i.e.*, to the right of the vertical, they go on in the same direction at first, but at a distance of about an eighth of an inch, each line curves upwards in a sweep of which you will see that the form agrees precisely in all the ten tracings. Having culminated, it descends to its previous height with a more gradual curve than that by which it ascended.

[1] In this experiment the opening shock is cut off by a mechanism not here described.

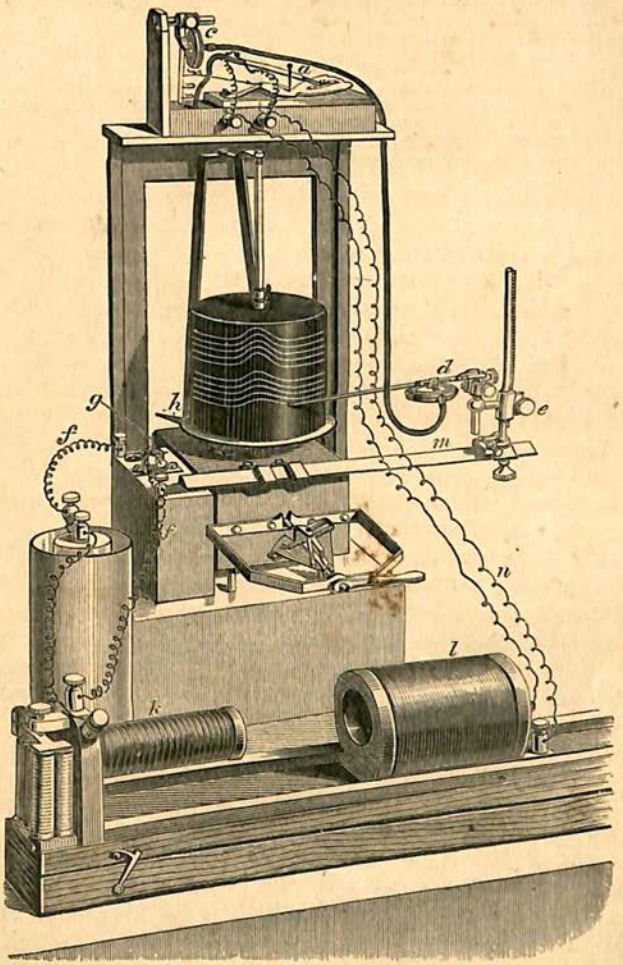


FIG. 9.—*a*, hind leg of frog. A silk cord is attached to the tendon of the muscle of the calf (*Tendo Achilles*), by which, when it contracts, it pulls upon the lever *b*, and thus compresses the air in the receiving tympanum *c*. Immediately below *d* the electrodes are shown, by which the induction current acts on the sciatic nerve. *d*, recording tympanum with its writing lever; *m*, steel bar on which it is supported; *e*, rack and pinion by which the recording tympanum can be lowered half a centim., after each experiment; *f f*, wires of battery circuit; *g*, trigger by which, as the arm *h* passes, the circuit is closed; *k*, primary coil of the induction apparatus; *l*, secondary coil; *n*, wires of the induction

circuit along which the instantaneous induction current, by which the sciatic nerve is stimulated, passes each time that the primary current is closed. Upon the cylinder are inscribed the records of ten experiments. The first five were written by the muscle in its normal state. In the last five it was becoming gradually cooled by placing near it a small block of ice. The last contraction has just been completed. The vertical lines by which the tracings are crossed were drawn before the experiments were made, by first bringing the cylinder into the exact position at which the arm A comes into contact with the trigger, and then working the rack and pinion. It indicates the point in each tracing which corresponds to the moment of excitation.

I need scarcely tell you that the curve is produced by the contraction of the muscle. It reveals to us that the apparently instantaneous effect which is produced, when a nerve is instantaneously excited, is in reality a process of three stages, each of which has a definite duration, viz. a period of latency which intervenes between the moment of stimulation and the commencement of change in the muscle, and corresponds to so much of the tracing to the right of the vertical, as is horizontal, and therefore to a period of about one hundredth of a second or thereabouts: Secondly, a period about three times as long, during which the muscle is contracting, and lastly a still longer period, during which it is returning to its previous condition.

As I before hinted, the knowledge of the mechanism and condition of muscular action is of fundamental importance in physiology. It is so because it is by muscular action that the most essential functions of our bodies are discharged, by it our circulation and respiration are maintained, and by it we maintain our relation with other persons and with the world around us. Muscle constitutes fully two-fifths of the weight of the body. By muscle, in a certain very true sense, we live and move and have our being; by muscle every action is performed, every thought expressed. It is therefore of great moment to the physiologist to be able to apply to the phenomena of muscular contraction more exact methods of research than to those of any other function; and this is so, not merely as regards the mechanism by which a muscle, when stimulated, shortens itself and thereby performs mechanical work, but quite as much as regards the molecular changes which in the living substance of muscle are associated with the change of form. Of all this the experiment I have shown you is a mere sample. It may enable you to understand something of the nature of the work in which physiologists are now engaged. Many vital

processes (among which may be mentioned specially those of the nervous system), are at present out of the reach of investigation. All that we can do is to stand on one side in the attitude of attentive observers. Investigation is impossible *because we cannot do that which constitutes an experiment*, we cannot modify at will the conditions under which the phenomena occur. As regards other processes our position as investigators is more favourable, and no better example can be given than that of muscular action. We can determine beforehand the conditions of an experiment, with as much exactitude as the physicist can in his investigations of the processes of the non-living world. I will illustrate this by a final example. We have here some ice, of which I will put a fragment near the muscle, of which we have already investigated the mode of contraction, and again record on the cylinder a series of contractions. The result is well worthy of your attention. Under the influence of cold, you will see that the process is completely modified. The curve of contraction, before so uniform, assumes, as the living substance of the muscle is gradually cooled, an entirely different character. Its function, however, though modified is not impaired. The proof of this lies in the fact that if the bit of ice is now removed, and the observation continued, it will gradually return to its former condition, *i.e.* the curve of its contraction will again present its normal contour.

APPARATUS FOR PHYSIOLOGICAL CHEMISTRY.

BY DR. LAUDER BRUNTON, F.R.S.

GENTLEMEN, you learnt from Dr. Burdon Sanderson yesterday the various modes of recording the movements of the body and its parts. To-day we have to consider how those motions are kept up. The old illustration that the steam-engine resembles the human body has become so hackneyed simply because it is so true, and because it affords us the best means of illustrating the manner in which the movements of the human body, or of any animal organism, are kept up. In both the animal organism and the steam-engine motion is kept up by the conversion of chemical into mechanical energy in the process of combustion. Now the products which are given off from the lungs are the same as those given off from the funnel of a steam-engine, viz., carbonic acid and water. You know that one of the most ordinary methods of ascertaining whether a man is dead or alive is to put a looking-glass before his mouth, and notice whether it is dimmed or not. This simply means that if the person is alive water is given off from the lungs, and water being one of the products of combustion, its condensation on the mirror affords a ready means of ascertaining that combustion is still going on in the body. But, although the movements of the whole animal are kept up by means of oxidation going on in the system, as you see from the products of combustion being given off by the lungs, yet Dr. Sanderson showed you yesterday that the muscle of a frog would continue to contract after it had been excised from the body. This proved to you that the

muscle was not dependent upon the lungs, the blood, or anything contained in the rest of the body for its power to contract, but contained in itself all that was necessary to convert chemical into mechanical energy. If you had put that very muscle into a test-tube, placed it over mercury, and analysed the gases given off from it, you would have found that, although separated from the body, it gave off carbonic acid; just as the whole body did, and this carbonic acid would have been increased if you had made the muscle contract. The muscle gives off more carbonic acid during the period of activity than it does during the period of rest. Combustion then goes on within the muscle itself. But this combustion does not go on exactly in the same way in the muscle as it does in the steam-engine, because if you entirely cut off the supply of air from the furnace of a steam-engine the fire will go out, but if you had put that muscle into a vacuum it would still have contracted, and would still have given off carbonic acid. There is, therefore, a likeness between the muscle and the steam-engine, inasmuch as they both produce motion by means of combustion, but they differ in the way in which the combustion goes on within them. In this respect, indeed, the muscle resembles a gun rather than a steam-engine, for it lays up within itself a store of oxygen by which it can keep up combustion for some time, just as gunpowder has oxygen stored up in its nitre, by which its charcoal is burned within the closed tube of the gun, yielding carbonic acid, and causing an explosion.

Indeed, the contractions of a muscle have been very fairly compared to a series of very small explosions, and each contraction may be regarded as a single explosion. But the muscle would soon use up all its store of oxygen, and it would cease to contract if fresh oxygen were not supplied to it. Yet how is the muscle to obtain oxygen, for it is a long way removed from the external air which is the only source of oxygen for animals? It obtains it by means of the blood, which conveys the oxygen to the muscles from the external air with which it comes into close relation in the lungs.

Blood will dissolve oxygen like any other fluid, but the quantity of oxygen which it could carry dissolved in this way to the muscles would be insufficient for their working,

and so arises the necessity for another arrangement, by which the oxygen-carrying power of the blood may be increased above that of other liquids. This is effected by means of the hæmoglobin or colouring matter of the blood. This is a very complicated body: it contains iron, and it may be split up into two bodies called hematine and globuline. It is one of the few animal substances of complicated chemical structure which are crystalline. It crystallises in the forms shown on this diagram. Its

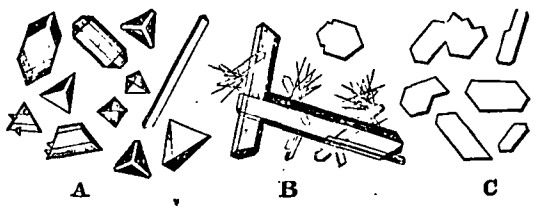


FIG. 1.—A. Hæmoglobin from guinea-pig's blood. B. Hæmoglobin from horse's blood. C. Hæmoglobin from squirrel's blood.

essential property is to take up oxygen readily from the external air, and give it off readily to any substance which is greedy of oxygen.

In this diagram of the circulation you see that the blood in the right side of the heart, and, in fact, the whole venous system, is coloured blue, while on the left side it is coloured red. These colours in the picture indicate corresponding differences in the colour of the blood itself. Although the blood in the veins is not quite blue, it is very dark red, whereas that in the arteries is light red. Now there are two points in the circulation at which this change of colour takes place, viz., at the heart in the centre of the body, and at the capillaries in the periphery. There is as marked a difference between the blood in the veins and that in the arteries at the periphery as there is at the heart, and this is due to the fact that the blood containing hæmoglobin has had its oxygen removed in the capillaries, whereas at the lungs it has had the oxygen again restored. This great distinction between venous and arterial blood has been long known, but it was not until the spectroscope was introduced as an instrument of research that we were able to ascertain with

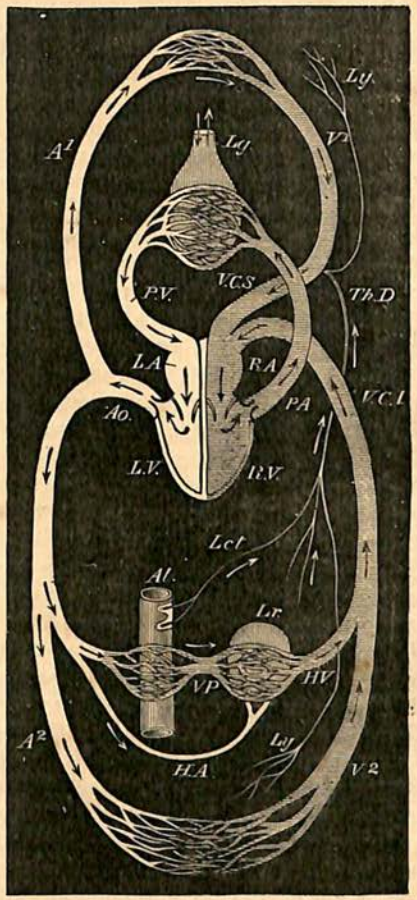


FIG. 2.—Diagram of the heart and vessels, with the course of the circulation viewed from behind, so that the proper left of the observer corresponds with the left side of the heart in the diagram. *L.A.* left auricle ; *L.V.* left ventricle; *Ao.* aorta; A^1. arteries to the upper part of the body; A^2. arteries to the lower part of the body; *H.A.* hepatic artery, which supplies the liver with part of its blood; V^1. veins of the upper part of the body; V^2. veins of the lower part of the body; *V.P.* vena portæ; *H.V.* hepatic vein; *V.C.I.* inferior vena cava; *V.C.S.* superior vena cava; *R.A.* right auricle; *R.V.* right ventricle; *P.A.* pulmonary artery; *Lg.* lung; *P.V.* pulmonary vein; *Lct.* lacteals; *Ly.* lymphatics; *Th.D.* thoracic duct; *Al.* alimentary canal; *Lr.* liver. The arrows indicate the course of the blood, lymph, and chyle. The vessels which contain arterial blood and which were coloured red in the original diagram are here light, while those which carry venous blood and were coloured blue in the diagram are here shaded.

any tolerable degree of exactitude to what the changes were due. Here I have a little blood mixed with water in order to render it transparent, as otherwise you would be unable to see its spectrum. I have here a small spectroscope which I will hand round, and will ask you, after directing the spectroscope to the sky, to place the two test-tubes in succession in front of its slit. First look at one spectrum and then at the other; one is arterial blood, and the other is blood which I have rendered venous, that is to say, from which I have abstracted the oxygen by adding a little of this solution. The spectrum of the blood was first investigated by Professor Stokes, and he imitated artificially the changes which go on in the tissues by adding to the blood a little of a fluid which will gradually abstract oxygen. This fluid consists of a little protosulphate of iron, to which some ammonia and tartaric acid have been added. When I add a little of this to the blood, it takes away the oxygen readily, and produces a change resembling that which takes place when blood becomes venous. It does not produce exactly the same changes in the blood that the tissues do; for although it abstracts the oxygen, it gives back no carbonic acid. You will find that in one spectrum you have two black bands, one in the orange, close to the solar line D, and the other at the beginning of the green; this is arterial blood. In the venous blood you find that these two bands have disappeared, and in their place is one band broader than either, which fills up the spaces between the places they would have occupied. If you were now to close the test-tube containing the venous bood with your finger, and shake it vigorously for a minute or two, so as thoroughly to mix the air and the blood (as I do with another specimen), and again examine the spectrum, you would find that the single band of venous blood had disappeared, and that the two bands of arterial blood had again made their appearance.

Now here I have imitated the changes which blood undergoes in passing through the body. When I added Stokes's fluid to it, I imitated the change it underwent in the capillaries; and when I shook it up again with air I imitated the changes that it undergoes in the lungs.

There are certain other peculiarities connected with this hæmoglobin. It forms, as I have told you, a combination

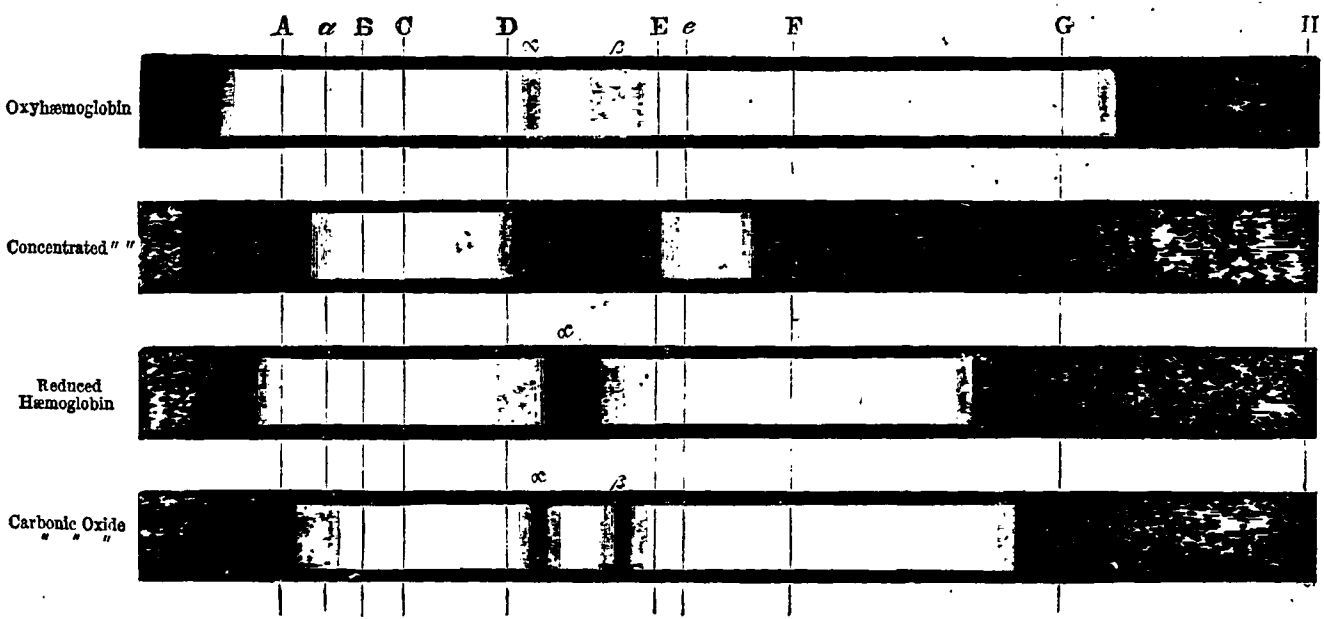


FIG. 3.—Spectra of blood. The first line shows the spectrum of arterial blood much diluted, the second shows that of arterial blood slightly diluted, the third shows that of venous blood diluted, and the fourth shows that of blood acted on by carbonic oxide and then diluted.

with oxygen termed oxy-hæmoglobin which is not very stable. It takes oxygen up from the lungs, conveys it to the tissues, and there gives it off; but it also forms combinations with other gases besides oxygen, and this is a point of very great practical importance. With carbonic oxide, the gas which is given off from charcoal stoves, it forms a combination very much resembling oxy-hæmoglobin in colour, but differing from it in this respect, that whereas oxy-hæmoglobin is readily reduced, carbonic-oxide-hæmoglobin is quite stable, and not easily broken up. In consequence of this property, if I were to take some blood saturated with carbonic oxide, which resembles ordinary arterial blood in appearance, except in being a little brighter, and shake it up with some of Stokes's fluid, I should get no change in it. The important point about this fact is, that when persons are confined in small rooms with charcoal stoves the hæmoglobin in their blood forms, with the carbonic oxide, this compound, which is no longer of any use for respiration; the carbonic oxide has displaced the oxygen, and there is no oxygen carried from the lungs to the tissues, so that the respiration of the tissues cannot go on. When persons are in this condition it is of no use to keep up artificial respiration, as would be the case if the person had fallen into a brewer's vat; the only remedy is to introduce some fresh blood. This was done in a case which occurred at Berlin some time ago. A person was found lying in a state of unconsciousness in a room with a charcoal stove. The doctor who was called to attend him found that he was so far gone that artificial respiration would be of no use. From previous experiments he knew exactly the condition of the blood, and at once proceeded to transfuse healthy blood into the patient's veins. The blood thus introduced was carried to the heart and lungs,—took up the oxygen, thence circulated to all parts of the body, and the man recovered perfectly. If it had not been for the previous experiments which proved that hæmoglobin when brought in contact with carbonic oxide forms this peculiar compound, that man's life would not have been saved. This affords a good example of the way in which experiments made purely from scientific curiosity and without any practical end in view afterwards become of the greatest practical use.

The interchange between the hæmoglobin of the blood and the tissues is called *internal* respiration, because it takes place quite away from the external air. It used to be supposed that combustion took place only in the lungs. This was, however, soon discovered to be erroneous, because, if so, the lungs would be the furnace of the body and ought to be very much hotter than any other part. This being found not to be the case, people began to investigate as to the part of the body in which combustion took place, and found that the venous blood returning from the muscles, glands, and other organs of the body contained more carbonic acid than the arterial blood distributed to them, and that therefore in all probability combustion took place in the organs themselves. But the question next arises whether combustion takes place in the cells, of which the various organs are composed, or in the small blood-vessels which permeate them? A partial answer to this question is given by the experiment which I have mentioned of the frog's muscle contracting and giving off carbonic acid when no blood is present, for this is sufficient to show that combustion has its seat in the muscles and not in the blood. But it is very probable that although the muscle gives off carbonic acid as a product of the combustion which takes place in it, yet that the whole of the material which is burnt in it is not given out in this form. For it would appear that just as in the steam-engine part of the fuel undergoes imperfect combustion and is passed out in the shape of cinders, which may afterwards be collected and burnt, so in muscle, there are certain products of imperfect combustion such as sarcolactic acid and glycerine-phosphoric acid which pass out into the blood and probably to some extent undergo further combustion in it. But in order to ascertain exactly what the changes are which go on in the muscle we must have recourse to quantitative analysis, and we require a more complicated apparatus, such as this instrument which is now before you (No. 3940 in the *Catalogue of the Special Loan Collection of Scientific Apparatus*, third edition). It is an air-pump by which we can pump out the gases from the blood. It consists of a receiver (*a*), which contains the blood, from which the gases are to be extracted, a bulb (*c*) filled with pumice-stone soaked in sulphuric acid to dry these gases, and an

exhausting apparatus for pumping out the gases. This exhausting apparatus is composed of a large fixed glass bulb (*a*) connected by a piece of india-rubber tubing (*d*) with a second bulb (*b*), which can be raised or lowered at pleasure by means of a winch and chain. At the upper end of *a* is a three-way stopcock (*g*), by which *a* can be entirely shut or be made to communicate at pleasure with

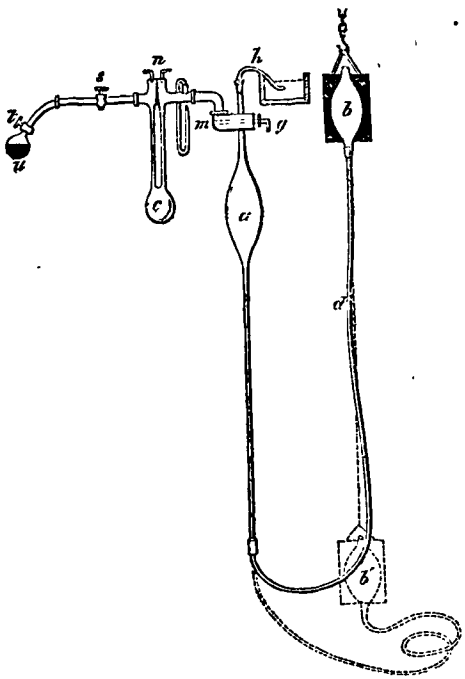


FIG. 4.—Mercurial pump for extracting the gases from the blood.

a tube (*h*) dipping into a pneumatic trough, or with the apparatus *u s c*. The bulb *b*, which instead of being somewhat smaller than *a*, as represented on the diagram, ought to be larger, is lowered to the position *b'* and filled with mercury. The stopcock *g* being then opened, the bulb *b* is raised by the winch to a higher level than *a*, so that the mercury flows from *b* into *a* until it is quite full, the air

escaping by the tube *h*. The stop-cock *g* is then closed, and *b* being again lowered to the position of *b'* the mercury flows back into it, leaving a vacuum in *a*. The blood is introduced into the receiver *u* by allowing it to flow in directly from a blood-vessel of an animal. It is then attached to the air-pump, but shut off from any communication with it by the stopcock *t*. The whole of the rest of the apparatus being then exhausted by raising and lowering *b* in the manner already described, or, as we may term it for convenience sake, by one or two strokes of the pump, the stopcock *t* is opened, and the gases of the blood rush into *a*. In their passage over the sulphuric acid in *c* they are dried, and by again raising *b* they are driven through *h* into a tube filled with mercury and placed over the pneumatic trough, but not represented in the diagram. A few more strokes of the pump extract all the gases, drive them into the tube just mentioned, and there they are analysed in the ordinary way, which it is unnecessary to describe here.

I mentioned to you at the beginning of this lecture that the muscle of a frog gives off carbonic acid during contraction, and that this evolution of carbonic acid can be ascertained easily and directly by simply putting the muscle in a tube and analysing the gases which the tube contains after the muscle has been some time in it. But you cannot find out by this simple and direct process that the muscle uses up oxygen during its contraction, for it does not take up oxygen to any extent from the air which surrounds it, but abstracts it from the blood which circulates through it, and stores it up within itself, so that it can continue to contract even after the circulation has been arrested. While the excretion of carbonic acid then can be examined directly, the absorption of oxygen by living muscle can only be investigated indirectly by examining the composition of the gases of the blood before and after it has passed through the muscle, and ascertaining whether the oxygen has diminished during the passage or not. On doing this it is found that the oxygen is diminished while the carbonic acid is increased, and by this means it has been finally settled that oxygen is used up and combustion goes on in the tissues, such as muscle, themselves, and not in the lungs or blood only.

We have next to ascertain how much oxygen is used up

by the whole body and under what conditions this oxygen

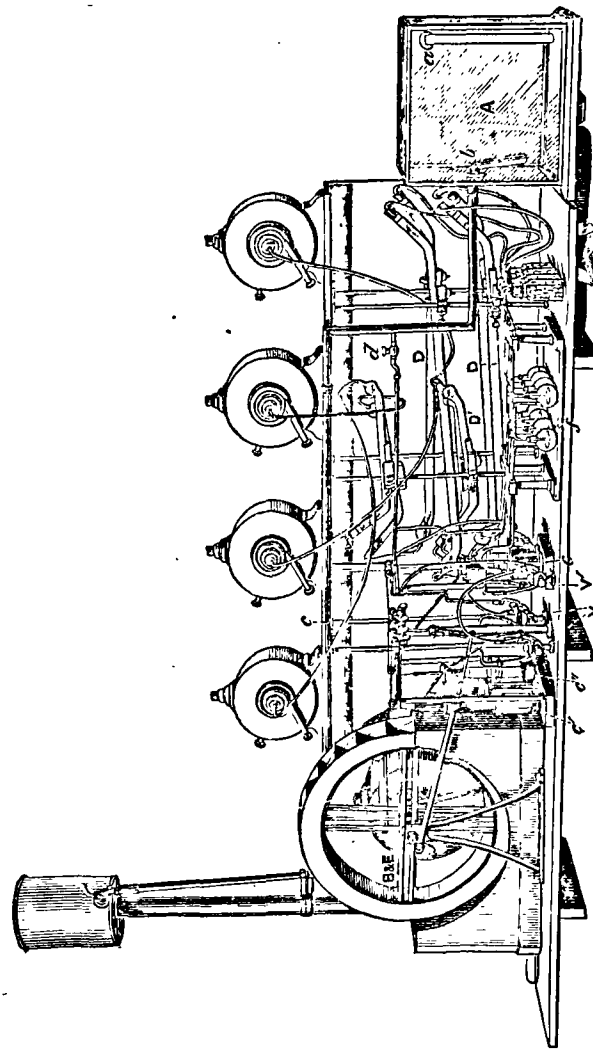


FIG. 5.—Voit's apparatus for examining the gases given off during respiration.

disappears, and is replaced by carbonic acid. Is the oxygen

used up at once as it is in a furnace or is it stored up in the tissues and given off afterwards? For instance, do I use up the oxygen that I get when I draw a breath immediately in keeping up muscular movements, or do I store up either the whole or a part of it in my muscles for use afterwards? This question is not to be answered by means of the air-pump which we have now been considering, but by another piece of apparatus which I now show you here, that of Pettenkofer and Voit, for estimating the amount of carbonic acid given off and of oxygen used up by any man or animal in a given time. Although this seems very large, it is really a very small apparatus of its kind. Pettenkofer and Voit have an enormous apparatus at Munich in which

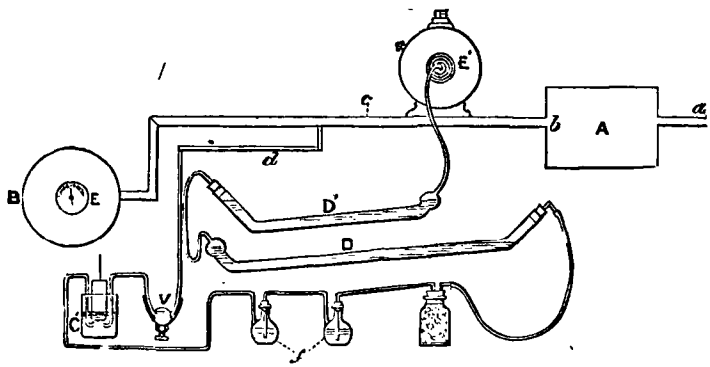


FIG. 6.—Diagram of Voit's apparatus. The letters indicate the same parts as in figure 5.

they can estimate the amount of carbonic acid given off and the oxygen used by a man. Here we can only use a small animal such as a rabbit (No. 3787 in *Catalogue*, third edition).

Now, complicated as this apparatus seems, it is in reality very simple in its plan. Here we have a glass box, and in it a rabbit. The box is rather small for the animal, and if it were completely shut, the box would very soon begin to feel very close, and the animal very uncomfortable, so, in order to prevent this, we must have a stream of air passing through it.

It is very difficult to analyse large quantities of air, and

it would be impossible to analyse the whole quantity breathed by a man during such a long time as an entire day. It is also quite unnecessary, for its composition can be ascertained as well from the analysis of a small sample as from an analysis of the whole, and if both the entire quantity of air respired and that contained in the sample be carefully measured, a simple sum in proportion will give the composition of the whole. The apparatus, then, consists of five parts.

1. A box, or room, A, in which the animal or man respires.

2. A fan, or pump, B, to draw a current of air constantly through the room.

3. Pumps, C, C′, C″, to suck out a sample of the air after it has passed through the room.

4. An apparatus, *f*, D, D′, to analyse this sample.

5. A large metre, E, to measure the total quantity of air which has passed through the room, and small metres, E, E′, to measure the samples.

There is an inlet in A at this side, *a*, through which the air enters, and an exit at the other side (*b*), through which it passes out. It is drawn through the apparatus by means of an aspirating machine, B, and the amount which passes is marked by means of a metre, E. We can thus ascertain the amount of air passing through the apparatus in a given time. The next thing is to analyse a sample of it. Here is the tube (*c*) through which the air is passing, and there is a little tube (*d*) attached to it, and if we can suck a little of the air through this tube we shall get a sample. This is accomplished by means of a little pump, C′, which pumps the air from the tube, and sends it through this apparatus, D, D′. This pump is neither more nor less than a small glass bell-jar, which passes into a vessel containing mercury. Each time it is raised it creates a vacuum into which the air rushes from the large tube leading from the box, and each time it is depressed it drives out the air again along another tube. It is prevented from going backwards by means of a little valve, V, consisting of a small bulb half filled with mercury, so arranged that the air can only pass one way. By having one of these valves here between the tube and the pumping apparatus, and another between the pump and analysing apparatus, we secure

that the air shall not go backwards, and at each movement of the bell-jar we withdraw a certain quantity of air from the tube (*c*), and drive it into the analysing apparatus, D′, and on through the small metre, E.

The first part of the analysing apparatus consists of a small jar filled with pumice-stone and strong sulphuric acid, *f*, which absorbs the watery vapour. This water, of course, increases the weight of the vessel, and as it is weighed before and after the air has passed through it, you can ascertain from the increase in weight how much water was contained in the air, and, therefore, by a simple calculation, how much water the animal was giving off in a certain time. The air is then drawn through a tube, D, containing a quantity of baryta-water, which absorbs the carbonic acid given off by the animal. From this long tube it passes into a shorter one, D′, which is filled, like the first, with baryta-water, and is simply supplementary to it, so that if by any chance the whole of the carbonic acid was not absorbed by the baryta-water in the first tube the second one would take up the remainder. The fact is, however, that the baryta-water in the first tube in almost every case absorbs the carbonic acid so completely that the second one is of no use. Then there is a small metre, E′, by which is measured the quantity of air which has passed through the small tube. From that metre you can either let the air go out into the atmosphere, or can collect it over mercury, and analyse it for oxygen. Thus you see you have the quantity of air passing through the apparatus measured by the large metre, then the quantity which is withdrawn for examination is measured by the small metre, and thus you know what proportion your sample bears to the original quantity. You may perhaps have a thousand litres passing through the apparatus, and ten litres taken as a sample. Then you can say that so much watery vapour has been passed off in these ten litres, and multiplying that by 100 you can say there was so much watery vapour exhaled by the animal in the time the apparatus was at work. By estimating the amount of carbonic acid collected by this baryta-water you can ascertain how much carbonic acid has been contained in the quantity of air passing through this tube, and multiplying that by 100 you get the quantity contained in the whole. These are the essentials of the apparatus. There is, besides,

another vessel which can be attached to each tube in front of the metre in order simply to saturate the air with watery vapour before it is measured. Thus we ensure that the amount of the watery vapour in the air and in the sample shall be the same. The volume of air differs slightly according to the amount of watery vapour it contains, and the water having been withdrawn from the sample it would be necessary to make a correction for the watery vapour in comparing its volume with that of the total air. A much easier plan is simply to saturate both the whole air and the sample of air with water, and then measure them.

But besides the means of measuring the total quantity of air passing through the apparatus, and of measuring and analysing the sample, you want a standard of comparison for your sample, and this is got by taking a little of the external air just as it enters the apparatus, and measuring and analysing it in the same way as the other sample. By comparing the amount of carbonic acid and watery vapour and oxygen contained in the air which enters the chamber with the amount of these substances contained in the air, after it has been breathed by the animal, you can make out what changes have been produced in it by the animal's respiration.

By means of this apparatus some very interesting facts have been made out; and I think one of the most interesting of all of these is that the oxygen we take in is not immediately used up, but may be used up a good while afterwards. For example, the oxygen that we take in during the course of the night is not used up at the time we are sleeping, but it is stored up for work the next day. During the night we take in more oxygen and give off less carbonic acid; during the day we take in less oxygen and give off more carbonic acid, so that the body as a whole can store up oxygen just as the frog's muscle of which I have several times spoken, and use it up as it is wanted. The day is the period of combustion, the night of repair, and the necessity for having well-ventilated bed-rooms becomes self-evident, for otherwise how can the body get the oxygen it requires?

I must now pass on to speak of the mode in which the muscles are supplied with fuel. We have hitherto been dealing only with the respiration of the muscles—of a single

muscle, or of the muscles of the whole body; but we must remember that the muscles, like a steam-engine, not only require oxygen, but fuel, and how are they to obtain this? The fuel is food, and in an ordinary meal consists, let us say, of a beefsteak, bread, butter, and sugar. Now, how are these to be dissolved, and how are they to be made available for the muscles? If you consider that they are in a solid condition, and that they have to pass through animal membranes, from the stomach and intestines into the blood—that they have to be in a state not merely of fine division, but in a state of very perfect solution, you will see it is not such an easy matter to provide the fuel for the muscles. Moreover, they require to be not only dissolved so as to allow them to pass into the blood, but broken up into other bodies having generally a simpler chemical composition so as to allow the muscles to assimilate them. A chemist could break them up by using strong heat or powerful acids, but neither of these means could be used by an animal body without undergoing destruction itself, and so the food is decomposed in another way which seems to me to bear the same relation to the violent methods of the chemist, that a plan of quarrying employed by the ancient Egyptians bears to the rude blows of a gang of powerful navvies. When those strange old people wished to cut an obelisk from a granite quarry, they simply cut a small groove in the direction in which they wished to split the obelisk. They drilled small holes in these grooves, and into the holes they put wooden wedges. Then they poured water into the grooves; the wooden wedges swelled up, and by their expansion burst the obelisk from the mass of living rock to which it was joined. Now foods are broken up in the animal body by substances termed ferments, which I think may be fairly compared to the wedges of the ancient Eygptians, as they do their work most efficiently, and with an expenditure of apparently very little force. For example, I have here some fibrine obtained by whipping blood. A chemist could decompose this by putting it into dilute hydrochloric acid, and then applying strong heat under pressure; but I can effect the same decomposition much more quickly without raising the temperature above that of the body if I simply add to the hydrochloric acid a little pepsin, the ferment contained in the stomach. The decomposition which occurs

during the assimilation of foods may be said generally to consist in their taking up water and then breaking up into two or more simpler bodies. The heat which the chemist applies may be said to drive the water in and tear them apart, while the ferment which accomplishes the same thing as the heat may be supposed to insinuate itself into them and let the water in so that they fall apart. You must remember that I use this illustration only in a general way, and that I do not mean to say it is in every way exact.

The ferments by which digestion is carried on act only at a certain temperature, namely, that of the body. When the temperature is low they hardly act at all, and above a certain temperature they are destroyed; so that it is only within certain limits that we can carry on digestion. The temperature of the body is constant, and if we wish to imitate the digestion which goes on in the body we must have a constant temperature. In order to do this we employ a thermostat or gas regulator (3950 *d* and *e* in *Catalogue*).

This is a sort of self-regulating valve through which the gas passes to the burner placed under the vessel we want to keep warm. The thermostat is put in the vessel, so that the temperature of both rise and fall together. The instrument is so adjusted that when its temperature falls it lets more gas pass through it, so that the flame becomes bigger and the vessel and thermostat consequently warmer. As the temperature rises the thermostat allows less and less gas to pass through it, the flame consequently becomes less and the temperature again falls. In this way the temperature can be kept oscillating within such narrow limits as to be for most purposes practically constant. There are various forms of thermostat, but in all of them the supply of gas is regulated by mercury, which expands when warm so as partially to block up the tube through which the gas passes, and on cooling contracts so as to leave it open.

In this way we are able to keep fluids at any given temperature for a long time together, and are thus able to imitate out of the body the process of digestion which goes on in it.

Now, as some of you may wish to repeat this experiment of artificial digestion, I may as well tell you how you can

get the ferments for yourselves. These ferments are contained in the stomach, in the pancreas, and in the small

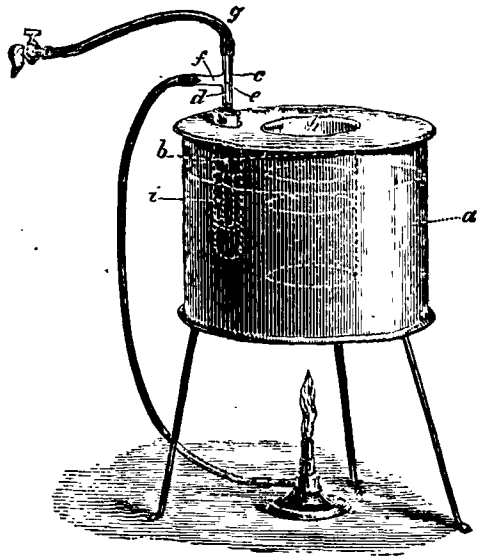


FIG. 7 shows a common form of apparatus for experiments on artificial digestion, furnished with one of Geissler's thermostats. *a* is a tin vessel nearly full of water, supported on a tripod and heated by a gas lamp placed underneath. *b*, *c*, *d*, *e* is the thermostat. *h* is a beaker, into which the substances to be digested are put. The thermostat consists of a wide tube, *b*, partially filled with mercury, furnished with a perforated cork, and a glass T-tube, *c*, *f*, inside which is a smaller and shorter tube, *d*, the upper ends of *d* and *c* being luted together. *c* is now pushed through the cork into the mercury in *b*, and connected by india-rubber tubing with the gas main and lamp. The gas passes from the main through the india-rubber tube, *g*, down the gas tube, *d*, out at its lower end into *c*, *f*, and thence to the lamp. As the mercury in *i* becomes warm it expands, and rising in *c* closes the lower end of *d*. The supply of gas to the lamp would thus be entirely cut off, were it not for a small hole, *e*, in *d*, which just allows sufficient gas to pass through to keep the lamp alight. The instrument is adjusted by putting *c* just so far through the cork in *b* that the mercury does not touch the lower end of *d* until the liquid in *a* is at the exact temperature desired. The thermostat would act perfectly well if *b* were filled with mercury only, but in order to render it still more delicate *b* is provided with a horizontal diaphragm in its middle, from the centre of which a tube passes nearly to the lower end of *b*. When mercury is poured into *i* some runs down to the bottom and shuts up the air in the space *i*, under the diaphragm. When the air in *i* gets warm and expands, it pushes up the mercury from the lower end of *b*, and closes the orifice of *d*. As it expands more quickly than the same bulk of mercury it renders the instrument more sensitive. It is sometimes difficult to get the hole, *e*, in the tube, *d*, sufficiently small. The temperature consequently rises above the required point, even when the end of *d* is closed by the mercury. To obviate this another form has been invented by Page, in which the gas required to keep the lamp alight passes through another tube, regulated by a stopcock, as shown in Fig. 8.

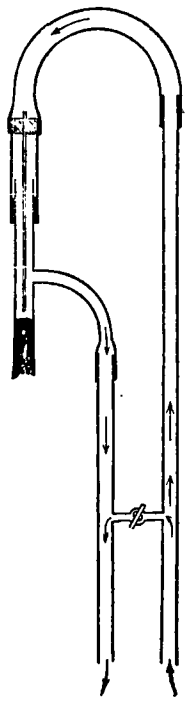


FIG. 8.—Page's thermostat showing the large tube and mercurial regulator through which the gas passes to keep up the temperature, and the small tube with a stop-cock through which the gas passes to keep the lamp alight when the supply is entirely cut off in the larger tube by the rise of the mercury.

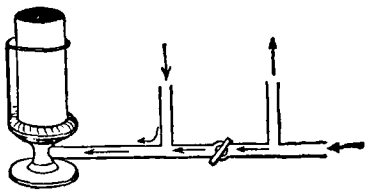


FIG. 9.—The lamp for Page's thermostat.

intestines. They are readily dissolved by glycerine, and the easiest way of getting them is to strip off a piece of the mucous membrane of the stomach, and to cut it

into small pieces, or to chop up a piece of pancreas and soak the fragments in alcohol for a few days, so as to coagulate the albumen. You then pour off the alcohol and pour on some glycerine, and allow it to stand. The glycerine dissolves the different ferments, and the albumen having been previously coagulated, you get a solution of the ferment tolerably free from albuminous matters. You can also buy the ferments of the stomach and pancreas. The substance sold as pepsin is obtained by simply scraping the mucous membrane of the stomach and drying it.[1] I do not know how the preparation from the pancreas is made, but both of these are most efficient; and it saves you the trouble of making them for yourselves. These ferments which exist in the body vary in their character. The first we meet with in passing down the alimentary canal is the ferment of the salivary glands. The action of this you can readily ascertain by simply chewing a piece of bread. It soon gets sweet, showing that the starch of the bread is becoming converted into sugar. The second is pepsin, the ferment of the stomach. This only acts in an acid solution. Here are two pieces of fibrine, which I put into these two flasks at the beginning of the lecture, and which have been kept at blood-heat in the water-bath. Both of them were treated with dilute hydrochloric acid, but to one of them a little powdered pepsin was also added. You see that the hydrochloric acid alone has simply caused the fibrine to swell up. But the hydrochloric acid and pepsin together have had a very different action. They have almost completely dissolved the fibrine. The solid parts have almost entirely disappeared, and the whole of it is dissolved just as it would have been in an animal's stomach. I might have treated a third piece of fibrine with water and pepsin alone for the sake of comparison, but it would have been of no use, except to show you that pepsin has no action whatever. You would have found that the fibrine remained exactly as it was before.

The next ferment is that from the pancreas, which has a threefold action. Like pepsin it dissolves fibrine, but it

[1] Pepsin may be obtained from Messrs. Bullock and Reynolds, Hanover Street, Hanover Square, London, and pancreatine from Messrs. Savory and Moore, Bond Street, London.

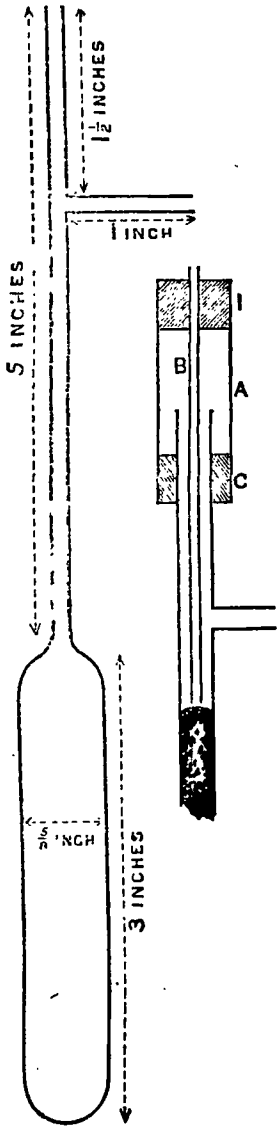


FIG. 10.—Page's thermostat. Instead of the ends of the tubes corresponding to *c* and *d* in Fig. 7 being luted together, they are surrounded by an outer tube, into which the mercury may run if it should rise too high.

also acts upon starch and converts it into sugar, even more readily than the salivary ferment does.

I have here in a test-tube a little fresh starch paste, and to this I add some solution of caustic potash and a few drops of a solution of copper sulphate. On boiling the mixture no change takes place in the colour. But if I now repeat this experiment with starch paste which has been allowed to stand in the water-bath for a short time after the addition of a few drops of glycerine extract of pancreas, the result is very different. The mixture instead of remaining blue becomes yellow and very turbid, and deposits an orange-yellow sediment. This is due to the reduction of the copper sulphate to copper oxide by the grape sugar, which has been formed from the starch paste by the pancreatic juice. The starch paste itself has no such action, and so the mixture containing it remains blue after boiling. The third action of the pancreatic ferment is upon oils and fats. It first of all forms with them an emulsion. Here is some oil, and if I add to it a little of this solution of pancreatine and shake it up, you will find it will form a milky emulsion, more especially if you add to it a little water. But this is not all, for it splits up the fat into fatty acids and glycerine, and the fatty acids uniting with some of the alkalis found in the intestines form soaps, and the soaps, along with the fatty acids and the unaltered particles of fat, pass readily through into the circulation; so that one of the chief uses of the pancreatic ferment is to render fat capable of absorption.

The only remaining ferment is that of the intestinal juice, and this is neither so good for demonstration, nor so easy of investigation as either of the other two. If you look into the subject, you will find that the action of the intestinal ferment is stated very differently by different observers. The reason of this, in all probability, simply is that its action is not so definite as that of the other ferments, and not so readily exerted upon the unaltered constituents of food, which we are accustomed to use in our experiments, as upon the residues which are left by the other ferments in the intestines. After the other ferments have acted upon the constituents of the food the intestinal ferment comes into action, and completes the work which they have begun.

By their action we get the albuminous constituents of the food converted into what are called peptones, soluble kinds of albuminous substances which differ very considerably from the other forms. We get starch converted into grape sugar, and both this and peptones diffuse readily through animal membranes, although albumin and starch do not.

In the collection we have a piece of apparatus which not only dissolves the food in the way I have shown you, but also shows you the passage of the food after solution through animal membranes. It consists of a truncated funnel, containing a filter made of a piece of bladder, and placed in a jar of pure water. Into the filter are put some fibrine, pepsin, and dilute hydrochloric acid, and the whole is then put in a water-bath at blood-heat. As the fibrine is digested, the peptones which are formed diffuse through the bladder into the water, and thus we get a good imitation of digestion in the stomach, where peptones are removed by absorption at the same time that more are being formed by the action of the gastric juice (No. 3496 in *Catalogue*).

There is only one other apparatus which I shall describe to you. Here you have a sheep which is kept constantly in one position. It may be fed regularly, and all the products of respiration may be collected if you like by putting the animal into a large Pettenkofer's apparatus. But the products of respiration form only one part of the products of the decomposition of tissues: a large part also passes out in the urine. Food contains a quantity of nitrogenous substances, and the waste products to which these give rise, after they have fulfilled their purpose in the body, pass out of it in the urine, and this must all be collected in order to ascertain what the decomposition of the tissues in an animal has been. The apparatus consists simply of a large iron case, in which the animal is kept fixed by straps, and in the vessel below the urine can be collected. The food can be collected as it is put in; and any food that is not consumed may be estimated; so that you can ascertain exactly the food that has been taken in a given time, the quantity of carbonic acid given off and the quantity of oxygen used; and you can collect underneath the urine, and estimate the quantity of nitrogenous waste given out in a given time. In this way you can

estimate the whole of the tissue change that has gone on in the body of an animal (No. 3939 in *Catalogue*).

There is one result of this I should like to mention. We could not know beforehand whether the decomposition of the nitrogenous substance went on in muscle and glands at the same rate as actual combustion. It might have been that combustion and decomposition of tissue went on at the same rate, but by experiments with this apparatus it has been found that that is not the case, but that, first of all, the muscle gets split up, and then that the products of the splitting up undergo combustion. The way this has been made out is by giving the animal phosphorus. It is then found that the nitrogenous waste is greatly increased, whereas the excretion of carbonic acid is considerably diminished. This shows that, although the tissues have been split up at a much greater rate than before, yet they have been undergoing combustion at a less rate than before. The consequence of this is, that instead of the animal remaining in a healthy condition, all the tissues, which were formerly nitrogenous, become more or less converted into fat. The reason of this appears to be that the albuminous bodies of which the muscles and glands are chiefly composed split up into urea and fat. The urea passes off by the kidneys, and the fat in a healthy animal undergoes combustion and yields carbonic acid and water, which pass off by the lungs. But when these albuminous bodies split up too quickly the whole of the fat which is formed in them cannot be oxidised, and so it remains in the place where it is formed until its accumulation gives to the organs of the body the appearance of having been converted into fat. A somewhat similar result is observed when oxidation is lessened in the tissues, even although they do not split up more quickly than they ought to do.

This explains a very curious fact which has been long known—that persons who have lost a great quantity of blood very often become extremely fat, and in some parts of Germany it is known even to the peasantry, so that when they want to fatten a cow rapidly, instead of giving it more food they bleed it, a very odd thing to do, one would say. The simple reason is, that by taking away a quantity of the blood-corpuscles, which usually carry to the tissues the oxygen which maintains combustion in them, the

oxidation of the non-nitrogenous substances such as fat is diminished, and the fat consequently accumulates. The splitting up of the tissues, which we now know to be independent of combustion, in all probability is carried on by means of ferments in the same way as the splitting up of food in the intestines. But here we reach the limit of our knowledge, and must wait until further experiments substitute certainties for hypotheses, and enable us to talk, not of probabilities but of facts.

ON EUDIOMETERS.

BY PROFESSOR MCLEOD.

THE name eudiometer was applied to the instruments employed shortly after the discovery of oxygen for measuring the purity of air, which was thought to depend solely on the quantity of oxygen it contained. The name has since been applied to apparatus used for the analysis of gases in general.

The first attempt at analysing air seems to have been made by Dr. Hales, and is described in his book on "Vegetable Staticks," published in 1727, and which is even now worthy of perusal, although the facts stated by him require a little correction. The apparatus Hales used was more for the formation of gas than the determination of its properties. The apparatus he employed was of this description: At the top was a glass retort, and to it was attached a flask with the bottom removed, called a bolt-head. Generally a glass retort was used, but when certain substances were used which required a high temperature, a gun-barrel was employed. The apparatus was filled with water up to a certain level by putting an inverted syphon into the bolthead, putting it in water, when some air escaped through the syphon. The syphon was then removed and a basin of water placed under the bolthead, when the water remained at a certain level. This level was marked by a piece of string, and heat was then applied to the retort, and in this way the gases appeared. There was a descent of the column of water in the vessel which was marked; and to show the

accuracy with which he conducted his experiments it may be mentioned that he determined the expansion of air in the retort when nothing else was present. He first warmed it, and generally found the water was displaced; then heating it to a low red-heat he measured the amount of expansion which took place under those conditions, and found it due to the difference in volume. On heating it to a bright white-heat the volume increased to three times. He then placed in his retort various substances, almost anything he could lay his hands on, organic or inorganic, and submitted them to what we should now call destructive distillation. The gases produced filled this bolthead more or less, and he noticed the volume at the end of the heating and after it had cooled, and then after allowing it to stand over water for some days. We know at the present time that when organic bodies are heated, one of the products is carbonic anhydride, which is soluble in water; and consequently in Hales' experiments the gas was continually disappearing, or, as he described it, it lost its elasticity; but still, as he thought he was dealing with common air, he never imagined he had anything but common air in these old experiments. He imagined that the gas had again become solid, or else that it was re-absorbed by the body left in the retort. However, he was a man of considerable intelligence, a philosophic clergyman. He was rector of Faringdon in Hampshire, and the minister of Teddington in Middlesex. He says at the end of his paper:—"If those who unhappily spent their time and substance in search after an imaginary production that was to reduce all things to gold, had instead of that fruitless pursuit bestowed their labour in searching after this much-neglected volatile Hermes, which has so often escaped through their burst receivers in the disguise of a subtle spirit, as mere flatulent explosive matter, they would then, instead of reaping vanity, have found their researches rewarded with very considerable and useful discoveries." This is so absolutely true that I can scarcely refrain from bringing it before you, although it is 150 years old.

The measurement of the goodness of the air seems to have been first attempted by Fontana about 1770, and afterwards by Priestley in 1774, and about the same time

also, or a little later, by Cavendish. These three observers employed for their experiments what was known at the time as nitrous gas, and which is now known as nitric oxide. When nitric oxide is brought in contact with air, or any gas containing oxygen, red fumes are produced, and these red fumes are easily absorbed by water, so that if the experiment be performed over water, and the two gases are mixed, there will be an absorption and diminution of volume due to the disappearance of the red fumes which have been absorbed by water. Let us try this experiment in a way somewhat similar to that in which it was done the first time it was tried. It was necessary to mix the air and the nitric oxide in equal proportions; and here I have a cylinder which I will invert over water, and which has upon it two strings to mark the height of the water. I first introduce this vessel full of air into the cylinder, and you see the water stands at the level of the first string. I could show you by introducing a second cylinder full of air that it would stand at the height of the second string, but perhaps you will take my word for it. Now I introduce an equal volume of nitric oxide as rapidly as possible; thus we mix the gases together, and allow them to absorb. You see the production of the red fumes. At the first moment there was a slight expansion, due to the heat, but now you notice there is an absorption, the red fumes are gradually disappearing, and the volume of gas is less than the sum of the two original volumes. This absorption was employed by Fontana, by Priestley, and by Cavendish in order to measure the goodness of air. It was supposed, especially by Fontana, that the greater the diminution of volume the better the air was, because it would contain more of this vital principle. However, it was soon shown that differences in manipulation were quite sufficient to throw out the results, and make them discordant. Fontana describes the way in which the gases are to be mixed exactly in two equal volumes, to be agitated for half a minute—no more and no less, with exactly the same amount of energy; and any difference in these details of manipulation were quite sufficient to spoil the analyses, and make them disagree with one another. It was in consequence of this disagreement that it was imagined that air was a body of variable composition. Cavendish, however, very much

improved the process by admitting the nitric oxide very slowly in minute bubbles into the volume of air with continual agitation, and in that way he succeeded in obtaining results very accordant indeed, but which were not capable of giving complete analyses of the air, because he also found, as has been recently found within the last few months, that the nitric oxide prepared by the action of acids on metals varies very considerably in composition.

Scheele used for the analysis of air another substance altogether, namely, a mixture of iron filings and sulphur made into a paste with water. This mixture was placed in a retort with the neck standing in water, and after several hours, or sometimes several days, the diminution of the volume of the gases or of the air was very perceptible. This is due to the absorption of oxygen by the iron filings and sulphur, but it was soon shown that after a time there is a formation of some sulphuric acid, which, acting on the iron, liberates hydrogen, and therefore there is ultimately a mixture of nitrogen and hydrogen left in the retort which gives a greater volume than the pure nitrogen which the air originally contained. This process of Scheele's had been previously tried by Dr. Hales. I tried an experiment of this kind during last week, and I find it takes three or four days with a small quantity of iron filings and sulphur, to absorb the oxygen from a moderately large retort. This process is, therefore, far too slow. Scheele also used a very ingenious mode of analysing the air, but one which is not capable of any direct application. He took a small bottle and placed it in some zinc and sulphuric acid so as to generate hydrogen. From the top of the bottle passed a narrow glass tube drawn out to a jet, at which the hydrogen was inflamed. This vessel was placed in a pneumatic trough, and a large glass was inverted over it as rapidly as possible. Of course the flask contained its own volume of atmospheric air. The neck of the flask was placed in the water of the trough, and of course, the hydrogen combining with the oxygen produced a diminution of volume. But he noticed the diminution until the moment the hydrogen was extinguished from the complete exhaustion of the oxygen, and then measured the minimum volume. Of course after that there began to be an increase of volume on the mixture of

hydrogen with the remaining nitrogen. This was a mode of analysing the air by the application of hydrogen, the process invariably used in all accurate experiments at the present time.

Another substance used by Scheele was a solution of liver of sulphur in water; that is obtained by fusing together sulphur and what used to be called potash, but which is really potassic carbonate. In this way he got a mixture of potassic sulphides and potassic hyposulphite, which is a mixture which has the power, especially when in solution and slightly warmed, of absorbing oxygen; and Scheele employed an instrument for the absorption of oxygen for this mixture, which was afterwards used by Guyton de Morveau, who employed the potassic sulphide at a high temperature placed in a retort with the neck standing in water. The end of the retort was ground into a long tube dipped into water. The retort was exactly filled with atmospheric air, and the ground tube was adjusted so as to prevent loss of gas by expansion in the retort by the first application of heat. Of course when heat is applied there is first expansion and then an absorption of oxygen, and the diminution of volume after cooling is measured.

Another great advance was made by Seguin, who used phosphorus for the absorption of oxygen. A tube was inverted over mercury, and into it was introduced a small piece of phosphorus. By placing a red-hot iron near the tube, sufficient heat radiated to melt the phosphorus against the side of the vessel. Air was then introduced, and the phosphorus being still warm was inflamed, and the oxygen was removed. The gas was then poured out into another graduated tube and measured. Berthollet also employed phosphorus, but used it at the common temperature, and the same process is still occasionally used. The objection to phosphorus is that it has a certain vapour tension at ordinary temperatures, and therefore $\frac{1}{40}$th of the residual volume must be deducted to allow for this quantity of phosphorus vapour. By using phosphorus in the cold state, six or eight hours are required for the complete absorption of the oxygen.

A great improvement was made in these processes by Volta, who proposed to explode a mixture of air and

hydrogen in a properly constructed vessel. Here is a modern example of a Volta tube. It is made of very thick glass with a stopcock at the lower portion, and is graduated into cubic inches. At the top are placed two platinum wires almost touching. The air and the hydrogen are collected in this tube over water or mercury; they are well mixed; the stopcock is then closed, and an electric spark passed between the wires, which produces an explosion. You have the hydrogen combining with the oxygen, the watery vapour is instantly condensed, and on opening the stopcock under water or mercury there will be a certain rise in the tube: the diminution in the volume will consist of two volumes of hydrogen and one of oxygen, and, therefore, one-third of the diminution will represent the oxygen present in the air. It is obviously necessary to employ an excess of hydrogen.

This instrument of Volta's was afterwards improved by Ure, and is known as Ure's Syphon Eudiometer, of which this is a modern example. It consists of a bent tube closed at one end with platinum wires sealed through it, graduated on the closed limb. When an explosion has to be made, the gas is introduced by filling the whole tube with mercury, and inverting it over mercury in the trough and allowing the gas to pass up in what are supposed to be the right proportions. You first put in the air, turn it over and measure the volume, and after having carefully levelled the mercury in both tubes, once more invert it, introduce the hydrogen, and measure again. The mixture is exploded by holding the tube in the hand, leaving a column of air in the open side, and closing the open end firmly with the thumb. The knuckle of the third finger is brought in contact with the wire, and by means of an electrophorus a spark is sent through the mixture. I never performed this experiment, because one imagines there might possibly be a little nervousness at feeling the electric shock passing through one's knuckle, especially when holding a tube containing gases to be exploded. I am told, however, that there is no danger whatever, that it only wants a little strength of mind, and that the concussion is entirely taken up by the cushion of air between the mercury and the thumb, and that the results are very concordant indeed.

Döbereiner, who discovered the curious action of spongy platinum on a mixture of hydrogen and oxygen, proposed to determine the oxygen in the air by introducing into a mixture of hydrogen and air spongy platinum made up into balls with clay. This gives concordant results, but there is a little difficulty about applying it, and it is therefore not much used, especially as we now know that the explosion method is superior to all others.

All those processes which I have described at present, refer to the determination of oxygen only, but this was not by any means all that was required; it was necessary to go very much further than to determine only one gas, and in the *Philosophical Transactions* for 1803, Hope described an instrument, of which there is a remnant here taken from the collection, of Dalton's apparatus. It consists of a graduated tube (the upper portion of which is unfortunately broken off) divided into ten parts, containing two cubic inches, and there is a small bottle fitted by grinding to the lower end. The bottle is filled with a liquid absorbent, which for oxygen might be a solution of *hepar sulphuris* or a solution of ferrous sulphate saturated with nitric oxide, which answers perfectly. This having been filled is closed with a ground-glass plate, and then placed in the pneumatic trough. The graduated tube containing air is put over the bottle, the plate removed, and the tube introduced into the ground neck. You then have a measured quantity of gas in contact with the liquid in a perfectly air-tight vessel. The apparatus is inverted and shaken about; it is introduced once more into the water, and the side stopper below is slightly opened; this causes an introduction of some water to replace the gas which has been absorbed, and the process is continued until no more absorption takes place. The residual volume in the case of air would be nitrogen, but this apparatus may be employed for the determination of other gases besides oxygen. Henry afterwards employed an india-rubber bottle instead of the glass bottle, which enables you to introduce the liquid absorbent into the gas with great ease.

But there was a great improvement made in this apparatus by Pepys, who made a number of most important experiments on gases in general, most of which he

described in the *Philosophical Transactions*. Here is one o Pepys' eudiometers also belonging to the Dalton collection. This contains one cubic inch of gas to the lowest division. There is a bent tube carefully ground in, and fitting so that when placed over the tube below there is no possibility of loss of gas. The india-rubber bottle attached to the bent tube is completely filled with an absorbent, and when so filled it is inverted and the tube fixed into the eudiometer which contains the gases under water or mercury. By simply squeezing the bottle you can introduce the reagent, squirting it up the sides of the tube and causing absorption. After the absorption is complete you may read off the 100th divisions with ease, provided the surface of the water or mercury is exactly at the division. But Pepys invented a very ingenious mode by which one-tenth of one of these small divisions might be read. The apparatus was a wide cylinder with a small opening at the bottom closed by a cork, through which a narrow glass tube drawn out to a fine point was capable of being moved. This was connected by means of a steel joint with an india-rubber bottle filled with mercury or water—generally mercury. The tube was drawn down below the level of the mercury in the vessel, then the eudiometer was placed over the narrow tube and depressed, or the india-rubber bottle pushed up until the point of the tube came just above the surface of the mercury within the eudiometer. Then by opening the stopcock a small quantity of liquid ran down into the bottle below, and raised the quantity of mercury within the eudiometer until it came exactly to the level of a division. When this was done, a certain portion of gas which corresponded to the fraction of the division had been introduced into this narrow tube. The narrow tube was graduated so that each division represented $\frac{1}{10}$th of the smallest division of the eudiometer, so that the eudiometer being divided into 100ths and this tube into $\frac{1}{10}$ths of those, it was possible to work to $\frac{1}{10}$th of a per cent., and Pepys' results agreed very accurately indeed with that small fraction.

A number of different absorbents have been used at different times for oxygen and other gases. The alkaline pyrogallate has been employed, and I shall presently show you an apparatus in which it is used. But it is to Bunsen

that we owe the greatest advance in the analysis of gases. We have on the table a fairly complete collection of Bunsen's apparatus, supplied by Desaga of Heidelberg. One of the instruments is what is termed an absorption tube, or absorption eudiometer. It consists of a tube about 250 millimetres in length and twenty in diameter. In this the absorption of different gases by reagents is carried out. Besides that you have an explosion eudiometer which is very much longer—800 millimetres in length—provided at the top with a pair of platinum wires, by means of which an electric spark can be passed through a mixture contained in it. The tubes are first graduated, but they are not divided, as was generally the custom in previous times, into certain volumes, but into lengths. They are divided into millimetres, and the apparatus by which the division is made is on the table. It consists of a double board with a graduated scale fixed on one side, and the tube to be graduated is placed at the other end. The tube is covered with wax, preferably mixed with a small quantity of turpentine. This is finely spread by means of a brush over the glass tube, which is then placed at the end of the apparatus. It is covered with two pieces of metal, one of which has a perfectly straight edge and the other has an edge with notches in at it every five millimetres, giving the long lines of the divisions. In order to graduate you use what is virtually a pair of beam compasses, a rod of wood with a point at one end and a knife-edge at the other. The point is placed in a scale, and by means of the knife-edge you make a scratch on the wax, and by this, in a short time, you get a tube of 800 millimetres graduated. After you have scratched the wax it is necessary to see that you have made no mistakes before the tube is removed. After having satisfied yourself that there are no mistakes, or correcting them by melting the wax by means of a red-hot platinum wire and re-marking it, the tube is removed, after the numbers are marked, by means of a steel pen, because that gives a double line in the down-stroke, which makes the graduation more easy to read. The tube is then placed over a trough of lead, containing a mixture of sulphuric acid and calcic fluoride—the hydrofluoric acid which passes off, etches the tube very evenly, and gives you divisions which are easily read. I

should like to say that I think the graduations on this tube of Desaga's are too long. I will give you my reasons for objecting to them, and then leave you to judge for yourself. I have put on the board five lines, which we will suppose to represent five divisions of the tube. When those are long, the interval between them appears to be less than when the lines are shorter. The lines are really placed about an inch apart, but they appear no doubt somewhat closer together. I will diminish the length of them, and I think you will find as the process of diminution goes on the apparent distance between the lines increases. This is really only an instance of the "scientific use of the imagination," but it is a very useful one. If I may be allowed to direct attention to one or two of my own graduations, you will find the length of the lines is only about a millimetre, that is to say the length is about equal to the distance between the two divisions, and I think in that way you get about the maximum apparent distance, which enables you to measure to about the one-tenth of a division without much difficulty.

The next process after the graduation of the instrument is the calibration of it; that is, you want to know the value of each division of the whole tube. The calibration is made by introducing measured volumes of mercury into the eudiometer. The eudiometer is placed in an inverted position truly vertical, being adjusted by means of a small plumb-line placed at the side of it. A glass tube, closed at one end, held in a wooden clamp to prevent change of temperature, and ground carefully at the upper surface so that it may be completely closed by a plate of glass, is filled with mercury by means of a little reservoir, the mercury being taken down to the bottom so as to prevent any air-bubbles being formed on the side. This little non-graduated tube is called the calibrating tube. When it is completely full it is removed and the glass plate forced on the top so as to expel any excess of mercury, and you then have the tube absolutely filled. This quantity is now carefully poured into the eudiometer which is to be calibrated. If any air-bubbles remain at the side they can be removed by shaking, or more advantageously by the introduction of a little piece of whalebone pressed against the side. After the addition of each volume of mercury

the height is read by means of a cathetometer, which consists of a telescope sliding on a vertical rod which has cross wires in the eyepiece, and enables you to get the exact surface of the mercury. Having got the instrument into its proper position, you read the divisions and estimate the 10ths at which the mercury stands. This process is repeated over and over again until the tube is full. If the tube were truly cylindrical each volume of mercury which is introduced would raise the column exactly to the same extent; but as a tube never is perfectly cylindrical, a calculation has to be made to determine the value of each separate division. When mercury is contained in a glass vessel the surface is not plane in consequence of capillarity —the surface is in fact convex. Now, during the calibration of the eudiometer the convexity of the surface is upwards, that is, towards the open end of the tube, whereas when the eudiometer is employed for measuring the volume of a gas, the curvature of the mercury is in the opposite direction. The volume of mercury which is measured by the calibrating tube is therefore a little less than the volume of gas that would be contained in the tube when the highest part of the surface of the mercury is at the same division on the eudiometer. It is necessary to determine this volume, which is done by noting the height at which the mercury stands when the eudiometer is inverted, and then introducing a small quantity of solution of mercuric chloride (corrosive sublimate), which attacks the mercury, altering the surface tension and making the surface horizontal. The distance which the mercury sinks in the centre of the tube is noted, and twice the volume indicated by the falling of the mercury is the volume which would escape measurement if this precaution were neglected. In all measurements made with the eudiometer this error of meniscus has to be added to the observed volume, or what comes to the same thing; twice the distance that the mercury has fallen is subtracted from the observed height of the column of mercury. The eudiometer is now ready for use.

It is of course extremely necessary in all measurements of gas analysis to be very careful to avoid parallax in the reading. If the eye is a little too high or too low you will read the instrument too high or too low. Thus I can,

by changing the position of my eye, read the mercury in this tube as high as twenty-four or as low as twenty-two. There are two ways by which this difficulty can be avoided; one is by the mirror method of observing. You take a piece of looking-glass and hold it against the tube, the tube being practically straight and the looking-glass being practically plane, the two surfaces will come in contact, and by placing the eye in such a position that the pupil which is visible on the looking-glass is divided into two parts by the surface of the mercury, you are quite certain that the eye is at the proper level. The only objection to this method is that you have to come so close to the instrument that the hand may raise its temperature; it is, therefore, preferable to use such an instrument as the cathetometer, which enables you to read from the other end of the room. This method of mirror-reading is introduced into one of the eudiometers I have here, in which the mirror is graduated instead of the tube, and you read off the level of the mercury by observing at the same time your eye and the surface of the mercury, and being quite certain that it is at the proper level.

The absorption of gases by Bunsen is carried out by means of solid reagents. He takes the reagent and fuses it in a small tube or crucible, and then pours it into a bullet mould. This mould is conveniently arranged with a small notch filed at the lower part, through which a platinum wire is passed, upon which the reagent is cast so that you obtain a wire with a bullet at the end of it. These wires here have on the ends bullets of caustic potash, and are used for the absorption of such gases as carbonic anhydride. I will show this by way of comparison, because afterwards I will show you the mode of absorbing the same gas by means of a liquid reagent. This is only a test tube—in proper gas analysis one would use an absorption tube, which is longer, and which would rest against this V-shaped support. This is a trough such as was used by Bunsen, made by Desaga, but it does not at all fulfil Bunsen's requirements. There must be a place on which the eudiometer is supported, so that when the gas is introduced there is no fear of the tube falling to the bottom. This is effected by a small projection with a notch in it, on which the tube will rest so that there is no

fear of its falling down. The next thing is to introduce the ball of potash, and it requires a little delicate manipulation to get it into its proper position without the admission of any atmospheric air. The potash must not be too hard. The bullet is placed below the mercury, and by means of the fingers the mercury is carefully rubbed against it so as to remove all traces of atmospheric air. It is then introduced into the eudiometer, and when it is in its proper position the end of the wire must be left below the surface of the mercury in order to avoid any possibility of any air being carried up by capillarity, between the mercury and the platinum. You notice how slowly the absorption is taking place, and this is pure carbonic anhydride, so that it is in its best condition to be absorbed, as there is always a supply in contact with the potash. When you are determining by means of caustic potash the quantity of carbonic anhydride which may be in a gas, it is necessary to leave the bullet in contact for several hours. When the absorption is complete the bullet is removed by a slight jerk, so that any gas carried down by it is left behind and not brought through the mercury. Then the absorption tube is placed perfectly vertical and left in a room by itself for at least half an hour, with a thermometer standing by its side. Then the operator comes into the room and stealthily approaching the cathetometer, so as not to produce currents of air, reads off the temperature of the thermometer and the height of the column of mercury. Having got this height he may more leisurely measure the height of the column of mercury in the trough on the outside of the tube. The tube being graduated almost down to the mouth, you can measure in this way the height of the column of mercury which it has in it, which has to be deducted from the barometer in the calculations. In all these experiments you must be very careful in reading the thermometer and also the barometer. The one used by Bunsen is a syphon barometer, graduated on one limb, and at the upper portion, it is divided from a zero point in the centre, and the column of mercury is determined by adding together the two readings on the tubes. Sometimes the zero point is placed below, and then you have to subtract the shorter column from the longer, but

there are less errors produced by addition than by subtraction in cases of this sort.

Of course you will see that the calculations necessary for determinations of this kind are something frightful. You have to measure the height of the column of mercury, the height of the barometer, to take into consideration the temperature of the atmosphere, and, in very accurate experiments, the temperature of the mercury column in the barometer. This is done by means of this small thermometer placed within it. In the case of the explosion eudiometer several precautions have to be taken. One which is essential is that at the bottom of the trough in which the explosion takes place there must be a pad of india-rubber against which the bottom of the eudiometer is placed. This is a eudiometer which I constructed some time ago, and which is a slight improvement on that of Bunsen, in this respect, that the wires do not project beyond the surface of the tube. If you have to deal often with a long eudiometer of that kind, and have to clean the outside of it, there is great danger of these pieces of wire being caught by the cloth and gradually bent one way or the other until they are broken: and very often, previously to the breaking of the wire you break the tube, from the slight cracking of the glass at the outside. In this one the wires have been cut off level with the surface of the glass, and then smoothed by rubbing on a piece of ground glass until you can scarcely feel their points. In this way there is no fear of the tube being cracked by the bending of the wire, and the contact is made by means of an ordinary American clip with two little pieces of platinum put within it to which wires are affixed, and to these you can hang the wires from the coil. If they break no harm is done. This tube contains a mixture of atmospheric air and hydrogen, and is arranged exactly as it would be for the determination of the amount of oxygen in the air. After the connections have been made, the tube must be held very firmly with the mouth pressed against the bottom of the trough on the caoutchouc pad. The explosion, unless you have an improper mixture, will not damage the tube; but you should hold it at the lower portion where the mercury is, because it is found that wherever such a tube bursts it is always at the surface of

the mercury. The only danger is that the face may be a little damaged by pieces of broken glass. You will notice a slight film of moisture on the tube when the explosion takes place, and that is all. You see the diminution of the volume, which has to be measured. Before the measurement takes place you must leave this eudiometer standing for at least three-quarters of an hour, so as to acquire the temperature of the air. Then you come into the room, measure off the temperature and calculate the result exactly as I described before. One precaution which I did not take in this case was to properly coat with mercury the surface of the india-rubber, which is best done by putting upon it a few drops of a solution of corrosive sublimate. This causes the mercury to adhere to the india-rubber, and then there is no danger, when contraction has taken place, of any air being sucked up. In order to save time I did not take this precaution, and it has had the advantage of forcing me to describe one point which is essential. When the explosion took place, it sucked up a small quantity of air from the surface of the india-rubber, and you saw the bubbles rising. All these processes require very great precautions. One thing necessary is to have the hydrogen very pure. The process employed by Bunsen for the preparation of pure hydrogen for the determination of oxygen is by an electrical process, in which the positive pole of the battery consists of some amalgam of zinc. This tube contains a platinum wire connected with one pole which is bent round below which will be covered with the amalgam of zinc, the rest of the tube being filled with dilute sulphuric acid. The negative pole, from which the hydrogen is evolved, consists of a plate of platinum. The apparatus is surrounded by a vessel of water which prevents a rise of temperature, and the gas when evolved is allowed to pass through this washing apparatus, which may be filled with sulphuric acid so that the gas is obtained in a dry state. The hydrogen can be prepared pure enough for ordinary purposes by the action of dilute sulphuric acid on zinc in a small apparatus, from which the air must, of course, be carefully expelled before the gas is collected. Occasionally it may happen that the gas which is being analysed contains so little oxygen that its mixture with hydrogen will not produce an explosive mixture, and in

such a case it is necessary to introduce some explosive gas obtained by the electrolysis of water to aid the explosion. The gases are generally measured moist; for this purpose a little water is introduced into the end of the eudiometer by placing a drop on the end of a long stick; this keeps the gas saturated, but great precaution must be observed with regard to the presence of water, for in some cases the gas is dry after your measurement, although it was wet before you began, so that you must make a correction for the tension of aqueous vapour in one case and not in the other. In this case the carbonic anhydride was quite wet, but after the potash was put in it will have dried it, and therefore after the removal of the bullet of potash it must be looked upon as a dry gas.

When the oxygen has to be determined, it is usually done by the diminution of volume by the disappearance of a certain quantity of gas after hydrogen has been introduced and the mixture exploded; but in some cases it is necessary to actually measure the steam. For this purpose Bunsen devised this instrument, consisting of a tube through which a quantity of steam from boiling water can be passed, in which the eudiometer is suspended by means of a holder after it has been removed from the trough: A powerful current of steam is sent through, and a thermometer is placed within so that the temperature may be determined; or you may assume it to be that of boiling water. The process is very rarely adopted, and perhaps I am not committing any breach of confidence when I say that a celebrated chemist who has done a great deal of work with gas analysis when he saw this instrument did not recognise it; which shows that it is not very much used.

Many improvements have been made in this process of analysis. You see the great slowness of it. The absorptions take a long time, and the time which has to be lost in allowing the gas to acquire the temperature of the air is very considerable, and the calculations also are by no means short, although they are not difficult. An improvement was made by Regnault and Reiset, who devised an apparatus in which the eudiometer is connected at the lower end by means of an iron three-way cock to another glass tube longer than but of the same diameter as the

eudiometer. At the top of the eudiometer is a capillary tube provided with an iron stop-cock and joint, to which a laboratory tube can be attached. The eudiometer and the lower part of the other tube (which is open at the top) are surrounded by a glass cylinder filled with water. The temperature being maintained constant by the water, the gas is measured in the eudiometer, which is provided with platinum wires to which copper wires can be attached, and an explosion can be made in the tube below the water, and readings can be taken off at once. The gases are introduced by means of a laboratory tube, as it is termed, which is placed in a mercurial trough, which may be raised or lowered by means of rack-work.

The difference of level of the mercury in the eudiometer, and second or pressure tube, will give very simply the height of the column supported by the gas, and this has to be introduced into the barometric calculation. The temperature is obtained by a thermometer standing in the water by the side of the eudiometer. The substance for absorption to be used will depend on the gas to be absorbed, but it is always used in the liquid condition. Whilst the gas remains in the measuring tube, a few drops of the liquid reagent are introduced into the laboratory tube by a bent pipette. The gas is then allowed to pass once more into the laboratory tube. The mercury is depressed, and the whole of the side of the vessel becomes moistened with the liquid, and presents a large surface for the absorption of gas which takes place very quickly. Let me by way of contrast show you the absorption of carbonic anhydride by liquid potassic hydrate. I take a small bent pipette with an india-rubber connector, and so introduce a little into the gas, and agitate slightly so as to moisten the side of the tube. You will see the carbonic anhydride very rapidly absorbed by the solution, whereas the former experiment, which has been standing now nearly an hour, is not yet complete. You see therefore the great advantage of the liquid reagent in the apparatus of Regnault and Reiset. This apparatus was afterwards very much improved by Dr. Frankland and Mr. Ward, who added to it as it now stands a barometer. You will see that as originally made this instrument would have to have all the barometric corrections made as

before; but by Frankland and Ward's apparatus the barometer tube was placed in the water-vessel together with the others, so that the pressures of the gases could be read off independently of changes of atmospheric pressure. At the bottom there was a means of communicating the tubes with one another in any way you pleased. At the bottom there is a three-way cock by which communication can be made between the two outside tubes, and a long barometer tube which passes down below. It is a tube of iron more than thirty inches long; so that when the stop-cock was turned in one position the mercury would flow out of the apparatus, and the whole become vacuous. The eudiometer was divided into ten equal volumes, so that it was easy to reduce the gas always to certain volumes, and then measure the tension or pressure in the barometer tube. In this way the corrections were much diminished, and there were fewer calculations to be made. We were entirely independent of the variations of the atmospheric pressure, and the only thing that had to be attended to was the constancy of the temperature of the water in the large vessel; but this was obtained by keeping a continuous stream of water flowing through the apparatus.

Some years after Frankland and Ward's apparatus was employed I used this one, which is absolutely on the same principle, although the supply tube was removed and the mercury was admitted by means of a reservoir which could be raised or lowered by a winch; there is at the back a black screen to enable you to obtain the proper illumination of the surface of the mercury. You get the bottom of the black line exactly on a level with the mercury and have the light below. The telescope here enables you to read off the height of the column, and at the same moment the volume of mercury is being altered by turning the stop-cock.

Dr. Frankland has lately improved upon his original apparatus by the removal of the centre supply-tube altogether, the mercury being admitted from a reservoir connected to the apparatus by a flexible tube. In this instrument we have the advantage of having no connections which are not of glass or which consist merely of two pieces of steel brought together. In Regnault and Reiset's you have metallic communication, there being steel stop-cocks and communications; and you have no idea of

the annoyance which is produced when in the middle of a complex analysis you see an abominable bubble of air rising up through the mercury in the eudiometer and mixing with the gas. Of this I have had several experiences, which made me vow I would never use steel joints when I could do without them. In a glass joint you can see immediately when anything goes wrong, whereas it is quite impossible to find where any part is out of order in a steel one.

Quite recently an apparatus has been described by Von Jolly, known as a constant eudiometer because he uses a constant volume. In Frankland's apparatus I told you there were eight or ten divisions of the tube by which the volume of gas was to be measured; but in Von Jolly's you have one only. In order to get rid of the temperature corrections Von Jolly employs a reservoir of ice placed round the vessel, and in this way you have only the barometric correction remaining. This barometer tube, at the top of which is the measuring vessel, is connected by means of a flexible connector with another tube capable of being slid up and down on the stand, so that by raising and lowering this, first roughly by means of this wooden screw and afterwards by a fine adjustment, you bring the level of the mercury absolutely to a level of the fiducial point at the bottom of the measuring vessel. There is one suggestion I would make to Professor Von Jolly, and that is to simply convert the open tube into a barometer, and you would have all the corrections complete at once.

There are many processes in manufacturing chemistry in which gases can be conveniently analysed, and there are on the table five or six different pieces of apparatus which are applied for these purposes. Here is one which has lately come into use, invented by Dr. Scheibler. It is an apparatus for the estimation of calcic carbonate in bone charcoal, and for the analysis of carbonates in general. A weighed quantity of the substance is submitted to the action of hydrochloric acid in a bottle connected by a caoutchouc tube with the measuring apparatus. The level in the two tubes is adjusted by introducing a liquid or allowing it to flow out. By graduating the tube in a different manner and by using a constant quantity of the carbonate you can determine the percentage of the carbonate instantaneously. Then there is an apparatus of Dittrich perfectly similar

in construction to Scheibler's, but it is simpler in one way because instead of having that complex arrangement for the raising and lowering of the liquid in the tube he makes one to slide, and you slide it until the column of liquid is level in both.

Then there is an apparatus described by Winkler, of which there are three specimens on the table. It is an absorption eudiometer; and unfortunately one instrument can only be used for the absorption of gas by one reagent without washing it out again; but by using two instruments you can use two reagents one after the other without loss of time. There is an ordinary stop-cock at the top, and at the bottom a three-way-cock. The gas can be blown through the measuring tube and out at the top. You can fill the apparatus simply by passing the current of gas through for a sufficient time. When the gas has displaced the air entirely the india rubber tube is closed by a compression cock, and you then have the eudiometer filled with the gas. The absorbent which is to be used is poured into this non-graduated tube, and a small quantity of air will be imprisoned between the long portion of the tube and the stop-cock. The three-way-cock is turned upside down, the compression-cock is opened until the reagent flows out through the nozzle; then you have the gas in one tube and the reagent in the other, and all that you have to do is to bring them in contact. You turn the three-way-cock when the column of liquid forces a small quantity of liquid into the gas. They are then well shaken together, and the stop cock is turned so as to allow the liquid to flow in. The readings are taken off in the usual way. When only a small quantity of gas has to be measured the lower part of the tube is graduated so that a small absorption which may be less than $\frac{1}{10}$th per cent. is shown. You see it would be impossible to employ this instrument for absorption by means of another reagent without transferring the gas into a second apparatus for a second absorbent, while washing out the first; but this instrument of Winkler has been used several times, and washed out without any accident occurring to it.

Then there is another apparatus of Max Liebig which is used for the determination of oxygen. The gas is measured in a pipette containing 50 cc., and connected with a caout-

chouc ball filled with water. By squeezing the ball the pipette is filled with water, and by allowing the water to return into the ball the gas is sucked in. After measurement the gas is forced into a flask surmounted with a graduated tube and filled with an alkaline solution of pyrogallic acid. The level of the liquid in the measuring tube and another parallel to it is adjusted by a bottle and caoutchouc ball.

Then there are three pieces of apparatus by Orsat, which are very ingenious indeed, and which all seem to be on the same principle. In Orsat's apparatus the gas is measured in a graduated tube connected by caoutchouc with a reservoir of water, mercury, or dilute glycerine. By means of a tube with several branches the gas may be passed into bell-jars containing the different reagents for the absorption of carbonic anhydride, a vessel containing a solution of caustic potash and a number of glass tubes open at both ends is employed. The tubes becoming moistened increase the surface. Oxygen and carbonic oxide may be absorbed by an ammoniacal solution in a bell jar containing copper-wire, gauze, or the oxygen may first be absorbed by an alkaline pyrogallate, and the carbonic oxide by the cuprous solution. Between each absorption the gas is transferred to the graduated tube and measured.

Here is another ingenious apparatus by which eudiometric work can be performed. Here is a small hydrogen apparatus by which hydrogen can be generated, passed into it and measured. Instead of producing explosion, which would destroy the instrument, the gas is slowly passed through a small spiral of platinum heated by a flame, and after having passed that it is transferred into a receptacle, once more brought back into the measuring tube and the diminution of volume measured.

Lastly, there is one other apparatus which I cannot pass over because it is entirely new and it appears to me to be capable of considerable improvement. It is an apparatus by Rüdorff. It is intended for the determination of carbonic anhydride in coal gas. It is a Woulfe's bottle with three stop-cocks ground in the necks, one being a three-way-cock. The three-way-cock is turned so as to connect one tube with the Woulfe's bottle, and a stream of coal gas is driven through from an inlet tube until you are quite sure that the whole of the air is expelled.

Having got it to this condition the stop-cock is turned off. Then a small quantity of solution of indigo is introduced into the manometer, the stop-cock is turned so as to connect the manometer with the reservoir. By opening one of these stop-cocks you will see that the level is easily adjusted, so that the gas contained within the vessel has the atmospheric pressure. When this is the case the stop-cocks are left in their present position. It is connected with a manometer, and into a graduated burette you introduce some solution of potash. You then turn the stop-cock and allow a drop to enter. The entrance of this drop produces absorption of the carbonic anhydride and there will be a rise in the level or diminution of the internal pressure. You admit more and more solution of potash until the pressure is restored, when you measure how much liquid has been introduced which is the actual measure of the gas you are absorbing. You will see that by changing the burette you can introduce three or four absorbent reagents one after another, provided that those first introduced do not interfere with the action of the following.

Messrs. Russell and West have devised an apparatus for the estimation of urea in urine. Five cubic centimetres of urine are introduced into a bulb at at the bottom of a wide tube, and the bulb closed with a stopper at the end of a glass rod. The wide tube is then filled with an alkaline solution of potassic hypobromite and water introduced into the trough, through the bottom of which the wide tube passes. A graduated tube is filled with water and inverted in the trough, the stopper is removed from the neck of the bulb, and the inverted tube placed over the wide one. The urea is decomposed by the hypobromite, and by gently heating the bulb the reaction is completed in five minutes. From the volume of gas collected the percentage of urea is determined.

There are many important chemical processes in which gases are produced, and a proper investigation of the quantities and composition of these gases, will doubtless be useful to the manufacturer as well as to the scientific chemist in following the changes taking place in various operations. All improvements in apparatus adapted to the analysis of gases are therefore of considerable importance from an industrial as well as a scientific point of view.

TECHNICAL CHEMISTRY.

BY PROFESSOR ROSCOE, F.R.S.

LECTURE I.

ON THE MANUFACTURE OF SULPHURIC ACID.

In his admirable Letters on Chemistry, Liebig gives it as his opinion that the commercial prosperity of a country may with great accuracy be estimated by the amount of sulphuric acid it consumes. You will readily acknowledge this to be true when you remember that there is scarcely an important branch of industry which, directly or indirectly, does not need to employ sulphuric acid for carrying on some of its processes, and when you learn that the result of this universal demand is that no less than 850,000 tons of sulphuric acid were manufactured in Great Britain last year, whilst this enormous amount is likely steadily to increase. By far the largest portion of this acid is employed in the manufacture, from common salt, of the alkali soda in its different forms, the remainder serving to carry on an endless variety of trades, amongst which those of the artificial manure maker, the gold and silver refiner, the candle maker, the dyer and bleacher, the calico printer, the lucifer-match maker, the wire drawer, the galvaniser, and the colour maker may be mentioned as some of the more important.

The national importance of this manufacture will now be obvious to you, and I propose to point out, as clearly as I am able, the chemical principles upon which this great branch of chemical industry depends, describing at the

same time the appliances, apparatus, and plant made use of in the production of this most useful substance. This is indeed all that any lecturer on Technical Chemistry can with wisdom propose to do. The attempt to teach manufacturing by means of lectures is, on the face of it, one which can only end in failure. But, on the other hand, to apply the principles of chemical science to an industry, and to explain the scientific basis upon which it rests, is not only a legitimate, but a most necessary part of our national system of scientific instruction, and one which ought by all means and at all risks most strenuously to be encouraged. For want of the general distribution of such a sound knowledge of scientific principles amongst our manufacturing and industrial population, the monetary loss to the nation, to put the case in its lowest terms, has been, and even yet is, enormous, and no portion of Government expenditure can be so remunerative as that employed for the purpose of spreading such a knowledge among the people; thus giving to manufacturers the means not only of conducting their known processes with efficiency and economy, but, what is even more important, enabling them to improve upon the old system, to introduce new and better means of attaining the end they have in view, and thus to bring about those revolutions in manufacturing industries, with examples of which the science of applied chemistry is so familiar.

Let us then first turn our attention to the history of our subject, and inquire when and how sulphuric acid became first known and applied. It appears probable that the celebrated Arabian alchemist Geber, who is said to have lived about the end of the eighth century, was acquainted with sulphuric, or, as it was formerly called, vitriolic acid, in an impure state. Basil Valentine, who lived in Erfurt at the beginning of the fifteenth century, was the first to describe fully the method of preparing this acid from ferrous sulphate or green vitriol, $FeSO_4 + H_2O$, and to point out that when sulphur is burnt with nitre, a peculiar acid, sulphuric acid, is formed.

As its common name, oil of vitriol, indicates, this acid was obtained in early times solely by the dry or destructive distillation of green vitriol. This salt is first roasted in the air, when it loses water, and becomes oxidized to a basic ferric sulphate, which can then be further heated in clay

retorts, as shown in Fig. 1, and yields a strongly fuming liquid, to which the name of fuming sulphuric acid is given, this, on addition of water, giving ordinary oil of vitriol.

Sulphuric acid at the present day is made by a totally different process. In order to understand the method as it is now carried on, it will be necessary for us to inquire into the chemical composition of the acid and the nature of the

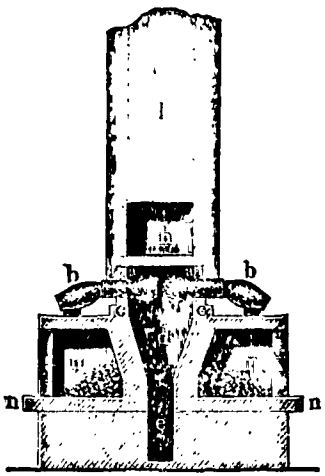


FIG. 1.

reactions which take place when sulphur undergoes oxidation. Sulphuric acid, like every other definite chemical compound, possesses a constant composition, which we indicate by the formula H_2SO_4, signifying that this body contains three elements, hydrogen, sulphur, and oxygen, united together in the proportion of two parts by weight of the first to thirty-two of the second, and sixty-four of the last constituent. When sulphur burns in the air, or in oxygen, it combines to form the lower of its two oxides, termed sulphur dioxide, and having the formula SO_2. This body, belonging to the class of acid-forming oxides, when brought into contact with water, unites to form an acid—

$$SO_2 + H_2O = H_2SO_3,$$

to which we give the name of sulphur*ous* acid. In order to obtain sulphur*ic* acid (H_2SO_4), by the combustion of sulphur, we must in some way or other add on to the dioxide a third atom of oxygen, and form the body known as sulphur trioxide (SO_3), and this, when it comes together with water, forms sulphur*ic* acid, as we see by the equation—

$$SO_3 + H_2O = H_2SO_4.$$

The problem, then, which we have to solve is how to get the sulphur dioxide to take up this additional dose of oxygen. There are several ways in which this may be done. I hold in my hand some finely-divided metallic platinum. I place some of the spongy metal in this tube, which I can warm with the lamp, and over this platinum I will pass a current of dry sulphur dioxide, mixed with dry oxygen gas. The former gas is evolved in the flask (*a*), and the oxygen passes in from a gas-holder through the tube (*b*). You observe that before reaching the platinum these gases retain their transparency, but that after passing over the metallic powder a dense white fume fills the tube and receiver. These white fumes consist of sulphur trioxide, and we only need to pass these fumes into water to obtain sulphuric acid. In this reaction the platinum remains unaltered, it simply enables the oxygen and the sulphur dioxide to combine together. This, you will say, is a very simple mode of making sulphuric acid, and one capable of being employed for the production of the substance on any scale. Unfortunately, however, this process does not succeed on a large manufacturing scale, for in practice the platinum becomes covered with dust and dirt, and its pores get stopped up, so that after a while it no longer fulfils its function, and is unable to bring about the required combination.

There is, therefore, nothing left but for us to look out for some other mode of effecting our purpose. It has long been known, as I have said, that when a mixture of sulphur and nitre is deflagrated, sulphuric acid is formed. This old observation serves as the key with which to solve our difficulty, and contains the principle upon which the modern manufacture of sulphuric acid is founded.

The apparatus which is before you illustrates in a satisfactory manner the chemical reactions which take place

in this process. In the bulb-tube I have placed some sulphur; I melt it with the lamp, and pass over it a stream

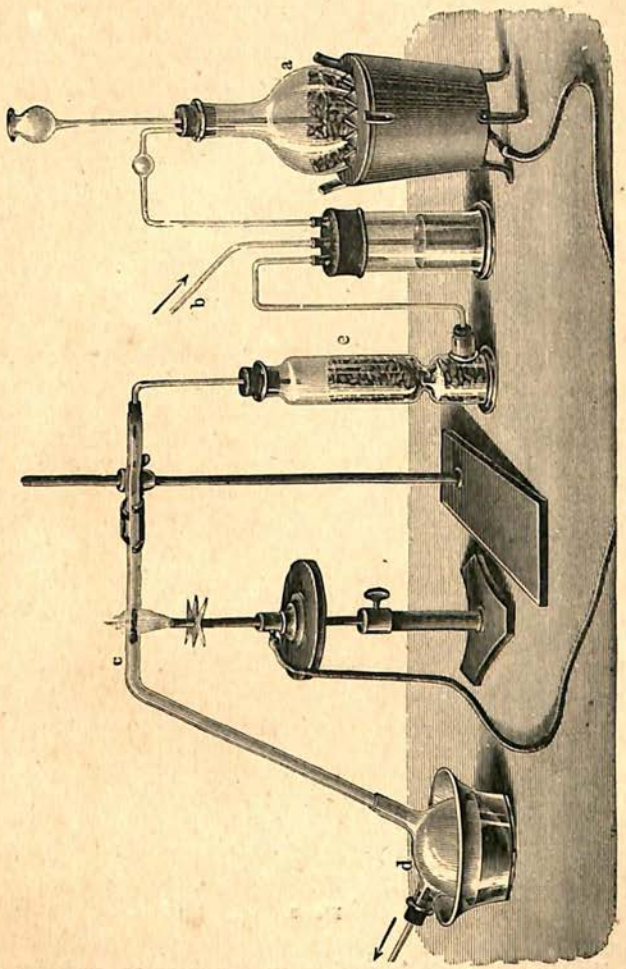


Fig. 2.

of air contained in the aspirator at one side. In a few moments the sulphur takes fire, burning with a lambent blue

flame, and the products of the combustion, the fumes of sulphur dioxide, (SO_2), pass, together with excess of air, into the large glass globe. So far, then, we have, as you see, no formation of sulphuric acid or white fumes. Now, how-

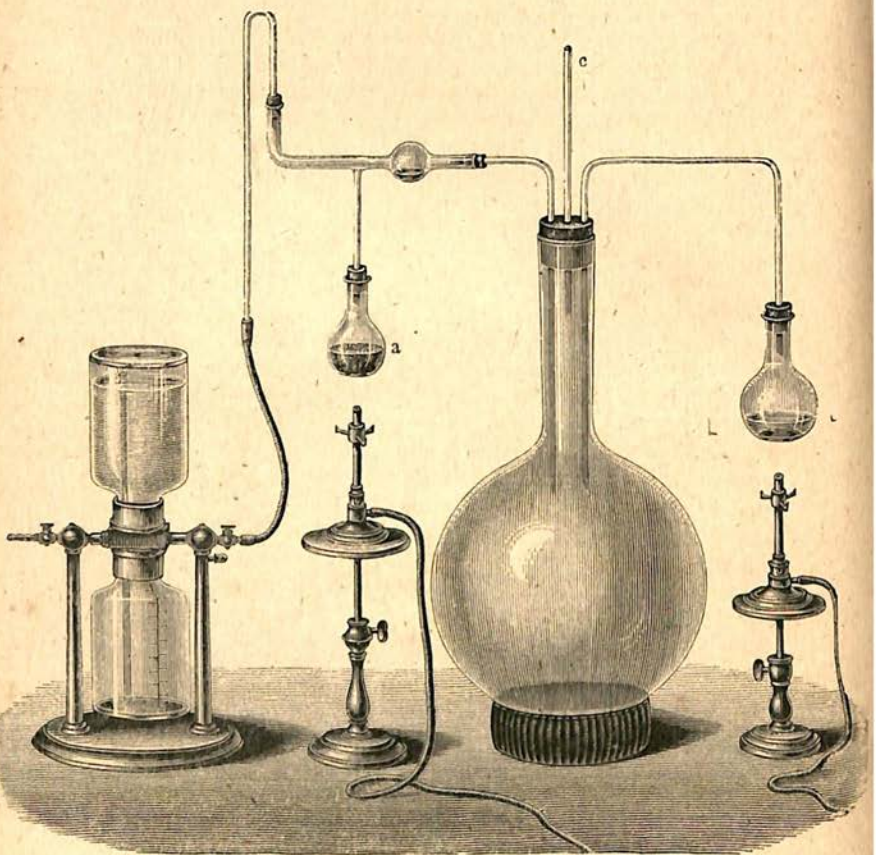


FIG. 3.

ever, I pour some nitric acid upon the copper contained in the small flask (*a*), and thus I evolve some nitric oxide gas, which passes into the globe along with the sulphur dioxide : at the same time I boil the water contained in the flask (*b*), and

the steam passes likewise into the globe. Under these circumstances we shall find that sulphuric acid is formed in large quantities in the globe.

Now for the explanation of its formation. Sulphur dioxide, as we have seen, cannot by itself take up oxygen from the air, but if any of the red nitrous fumes, such as nitrogen peroxide, (NO_2), be present together with water, then the sulphur dioxide can take up oxygen from the red fumes, and thus sulphuric acid is formed. This is expressed by the equation—

$$SO_2 + H_2O + NO_2 = H_2SO_4 + NO.$$

The moment that the nitric oxide, NO, one of the products of this reaction, comes into contact with the free oxygen of the air, it combines to form the peroxide of nitrogen again, thus :—

$$NO + O = NO_2,$$

and this peroxide is ready again to give up half its oxygen as soon as it meets with sulphur dioxide and water. So that, as you will readily understand, an infinitely small quantity of red fumes is able to convert an infinitely large quantity of sulphur dioxide, oxygen, and water into sulphuric acid, for these red fumes simply serve as the carriers of the atmospheric oxygen to the sulphur dioxide. This is, then, the theory of the manufacture of sulphuric acid, so far as we yet understand it.

Let us now pass on to the practical part of our subject, and trace the progress of the manufacture from the earliest and most rude methods up to the most perfect plans in use at the present day.

Cornelius Drebbel appears to have been the first to introduce the present system of manufacture into England, but the first satisfactory information concerning the methods adopted was given by a quack doctor of the name of Ward, who shortly after the year 1740 introduced into England a process for making the acid, originally proposed in France by Messrs. Lefèvre and Lémery. This consisted in burning a mixture of sulphur and nitre in a large glass globe or bell containing water. The globes were of some forty to fifty gallons in capacity. A stoneware pot was introduced, and

on this a red-hot iron ladle was placed. The mixture of sulphur and saltpetre was then thrown into this ladle, and the vessel closed, in order to prevent the escape of the vapours which were evolved; these were absorbed by water placed in the globe, and thus sulphuric acid was produced. From the mode of its manufacture this was termed acid "made by the bell," and was sold at 2*s.* 6*d.* per pound. In 1746 a marked improvement was introduced into the process by the substitution of the glass globe by a leaden vessel or chamber about six feet square. This was effected by Dr. Roebuck, of Birmingham, and in 1749 works for the manufacture of sulphuric acid by this process were erected at Prestonpans, on the east coast of Scotland. The mode of working these leaden chambers was similar to that adopted with the glass globes. The charge of sulphur and nitre was placed inside the chamber, then it was ignited, and the door closed. After some time, when most of the gases had been dissolved by the water contained on the floor of the chamber, the door was opened, the remaining gases allowed to escape, and the chamber then charged again.

For many years the chambers did not exceed ten cubic feet in capacity, and yet in these all the acid used in the country was made, and much was exported to the Continent, and known as "English sulphuric acid," a name still in use to distinguish acid thus made from that obtained by distilling green vitriol, and known as "Nordhausen acid," from the place of its manufacture.

The first vitriol works in the neighbourhood of London were erected at Battersea by Messrs. Kingscote and Walker in the year 1772, whilst eleven years later a connection of the above firm established sulphuric acid chambers at Eccles, near Manchester. The number of these works soon increased, and in the year 1797 there were, according to Mr. Mactear, no less than six or eight different sulphuric acid factories in Glasgow alone, whilst England, which hitherto had been a large importer, in the above year exported oil of vitriol to the extent of 2,000 tons.

The object for which much of the acid was at that period employed was as a substitute for sour milk in the old system of bleaching, by which at least half the time needed for the bleaching operations was saved. Then again a great stimulus was given about this time to the

manufacture by Berthollet's application of chlorine to the bleaching of cotton goods—sulphuric acid being needed for the formation of this element; and from that time up to the present the demand for sulphuric acid has gradually increased, until it appears now to be almost unlimited. According to the valuable statistics of Mr. Mactear, the cost of manufacture in the year 1798 was 32*l.* per ton, and the selling price from 50*l.* to 60*l.*

The next improvement consisted in making the operation continuous. For this purpose a brick furnace was attached

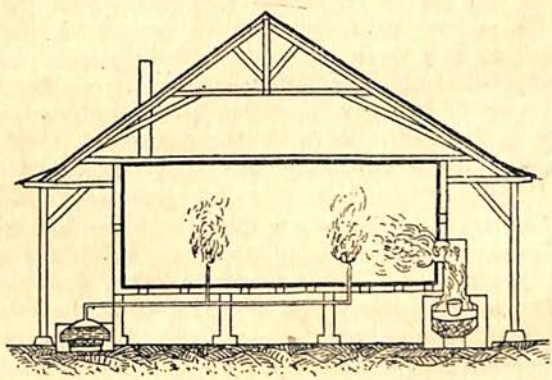


FIG. 4.

sometimes to one, and sometimes to two chambers, as shown in Fig. 4. In this the mixture of sulphur and nitre was burnt, the gases first passing into one, and afterwards being allowed to pass into the other chamber. One hundred parts by weight of sulphur was thus made to yield 200 parts by weight of sulphuric acid, and to need fifteen per cent. of nitre. According to theory, 100 parts of sulphur can yield 306 parts of pure sulphuric acid, and it is now found possible to obtain by careful working no less than 300 parts of acid, whilst only four parts of nitre are needed. From this you will see how accurately it is now possible to carry on the process. About the year 1814 jets of steam were thrown into the chamber instead of water being placed on the floor, and from that time forward the system of manufacture has undergone no essential

change, though many improvements in the working of the various parts have been made. On the preceding page you have a drawing of the old form of sulphuric-acid chamber, whilst in Fig. 5 you find a bird's-eye view of a more modern sulphuric acid works. These leaden chambers, of which two are represented on the figure, are built of sheet lead (seven pounds to the square foot), soldered together by melting the edges of two adjacent sheets by means of the oxy-hydrogen blowpipe. The chambers are supported on a wooden framework, to which the leaden sheets are attached by straps of lead, and the whole is raised some ten or twelve feet from the ground by pillars of iron or brickwork. In general, the whole erection is protected from the weather by a roof, or at any rate by boards which serve to keep off most of the rain. The sulphur or sulphur ore is burnt in suitable ovens or kilns (*aa*), and the gaseous product is led, together with atmospheric air, into the chamber, whilst ferric oxide (Fe_2O_3) remains behind in the kiln. For the purpose of obtaining the nitrous fumes, which, as we have seen, are necessary for the process, a small stove (*b*), containing nitre and sulphuric acid, is placed in the central portion of the kiln. In this stove the nitre is decomposed, an alkaline sulphate being left behind, whilst the nitrous fumes evolved pass, together with the other gases, into the chamber. Jets of steam are also blown into the chamber at various points from a boiler (*c*), and a thorough draft is maintained by connecting the end of the second chamber with a high chimney not shown in the drawing. The sulphuric acid as it forms falls on the floor of the chamber, and when the process is working properly it is continually drawn off as it attains a specific gravity of 1·55, or contains 64 per cent. of the pure acid, H_4SO_4, the rest, 36 per cent., being water. In order to obtain an acid stronger than this, further operations of concentration and rectification must be performed. The acid of the above strength, called chamber-acid, cannot be strengthened in the chambers themselves, because an acid stronger than this has the property of absorbing the red nitrous fumes, whilst the weaker acid does not do so, and the presence of such a strong acid would therefore effectually prevent the further working of the chambers. That this is the case I can readily show you. Here I have some strong sulphuric

acid; into this I will pass a current of these red nitrous fumes. You see that they are absorbed, but if, after allowing the nitrous fumes to pass in for a few minutes,

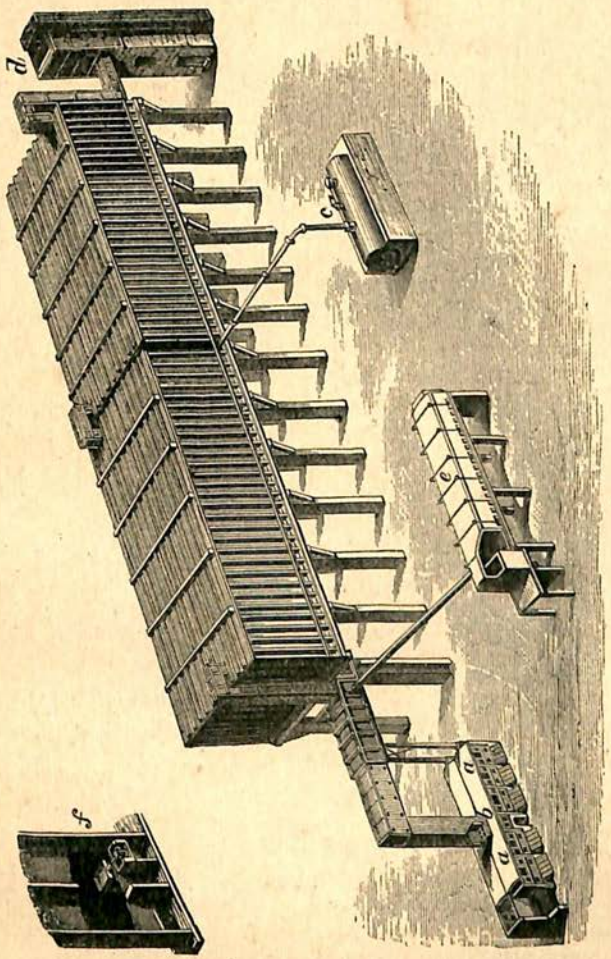


FIG. 5.

I now pour some water into this "nitrated acid," the red fumes are, as you see, evolved in quantity. Remember, if

you please, this experiment, as we shall need hereafter to make use of this property.

Now I think we have got a clear idea of the general nature of the manufacture. Let us next pass to consider in detail the several parts of the process. First we will begin with the raw materials. Up to within the last thirty years all the sulphuric acid manufactured was made from sulphur imported into this country from Sicily, where you know it occurs in the native state amongst the volcanic deposits of which that island is the seat. The price of Sicilian sulphur up to the year 1838 varied from 6*l.* to 8*l.* per ton, but in that year the King of the Two Sicilies attempted to establish a monopoly, which had the effect of raising the price of sulphur to 20*l.* per ton. This senseless action, however, soon worked its own cure, for the manufacturers beginning to inquire about other and cheaper sources of sulphur, found that a sulphur ore known as pyrites, and consisting of bi-sulphide of iron (FeS_2), and usually containing small quantities of sulphide of copper, could be obtained in large deposits in Ireland, Spain, and elsewhere, and that this, when burnt in kilns, yields sulphur dioxide precisely as sulphur itself does, but at a far cheaper rate, as the mineral is abundant, and is obtained without much cost.

Another great advantage possessed by this pyrites is that its use as an ore of sulphur establishes a new industry, viz., the extraction of the three or four per cent. of copper which the burnt ore contains. This has now become an important trade, and when you learn that at least 500,000 tons of pyrites containing from three to five per cent. of copper is annually burnt in England, you will be able to appreciate the importance of this new source of copper.

A sectional view of a series of three sulphuric-acid chambers is seen in Fig. 6. This likewise exhibits the construction of the pyrites kilns; whilst in Fig. 7 the detailed arrangement of such a burner is seen. The pyrites burns when it is once kindled by throwing the stone into a kiln previously heated to redness, and a second charge is brought into the furnace and ignited whilst the first is still hot, so that a constant supply of sulphurous acid to the chamber is kept up by charging the burners in regular order. The ordinary charge of pyrites is about five

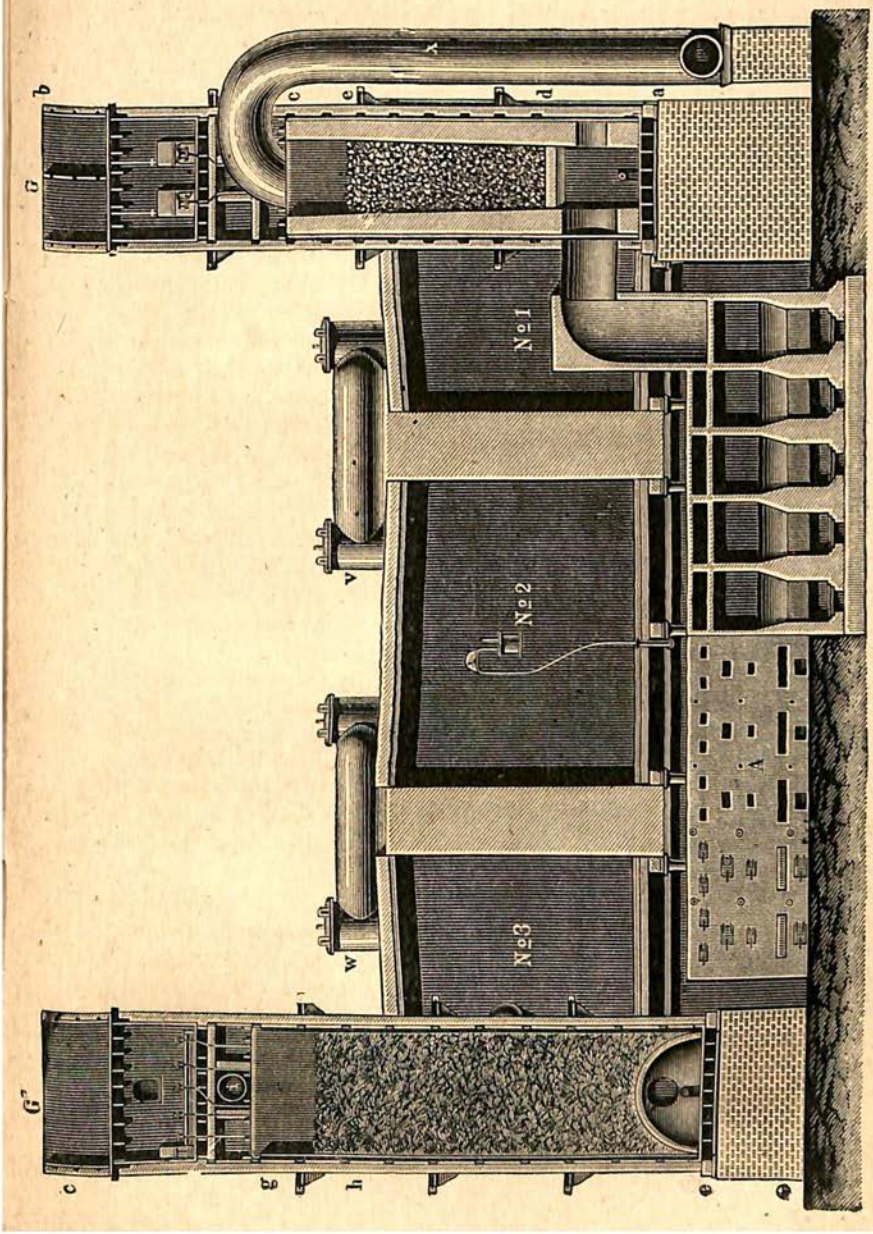


FIG 6.

to six hundredweight for each burner; the pyrites contains usually about 48 per cent. of sulphur, and the charge is

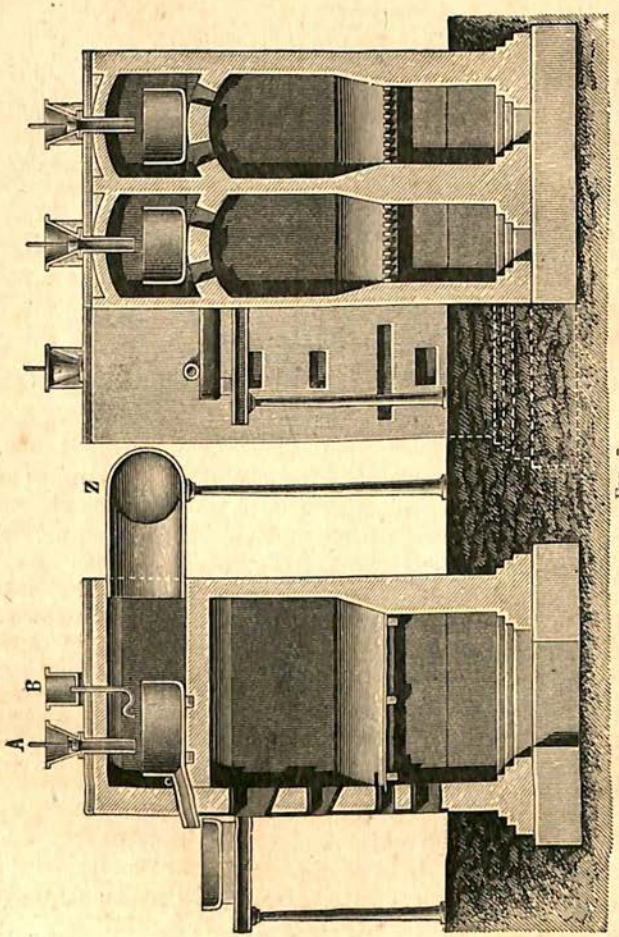


FIG. 7.

burnt out in twenty-four hours. In order to ignite a fresh kiln, the kiln is heated to redness by ordinary fuel, and the charge of "stone" thrown in. The following table

gives the analyses of several samples of pyrites now largely in use.

ANALYSES OF IRON PYRITES.

	Spain.	Belgium.	Norway.	Ireland.	Cornwall.	Coal Brasses.
Iron	8 70	39·68	39·22	40·52	32·20	31·44
Sulphur	44·60	45·01	45·50	44·20	34·34	38·10
Copper	3·80	—	1·80	0·90	0 80	Trace
Lead	0 58	0 37	—	1·50	0·40	—
Zinc	0·30	1·80	1·18	3·51	1·32	—
Thallium	Trace	Trace	-	—	—	—
Lime	0·14	0·25	2·10	0·24	—	4·96
Magnesia	—	—	0·01	—	—	0·33
Carbonic acid ...	—	—	1·65	—	—	5·11
Arsenic	0·26	Trace	—	0·33	0·91	Trace
Oxygen	0·23	0·42	0·45	0·25	—	0·31
Coaly substance ...	—	—	—	—	—	14·45
Gangue	11·10	12·23	9 08	8 80	29·00	1·40
Moisture	0·17	0·25	0·17	0·90	—	0·90
	99·88	100·01	101·16	101·15	98·97	97·00

By far the greater portion of the 850,000 tons of acid made every year in Great Britain is what is termed "stone acid," that is acid made from pyrites. But the employment of this source of sulphur has its disadvantages. As you see in the diagram, arsenic is a common ingredient of pyrites, and when the pyrites is burnt the arsenic is oxidized as well as the sulphur, and the trioxide of arsenic, or white arsenic, as it is termed, is carried forward into the chamber, and thus it comes that almost all the sulphuric acid of commerce contains traces, and often far more than traces, of this poisonous substance.

Near the pyrites burners the nitre pots, or ovens, are generally placed. In these a charge of 30 lbs. of nitrate of soda, or Chili saltpetre, and 22 lbs. of sulphuric acid of specific gravity 1·75, is placed, and after the lapse of two hours, when the charge is exhausted, the fused sulphate of soda is run off into a pan placed outside the oven. The decomposition of the nitre as shown by the equation

$$2NaNO_3 + H_2SO_4 = 2HNO_3 + Na_2SO_4$$

is assisted by the heat from the pyrites burners placed

below, and the nitric acid fumes which are evolved pass into a cast-iron pipe, which discharges its contents into the long horizontal flue carrying the products from the pyrites burners into the chamber.

Here I may digress for a moment to remind you that the enormous demands of the English sulphuric acid manufacture has introduced industry and wealth into an arid district in South America. In the rainless district of Atacama, in Peru, a deposit of a white salt was discovered some years ago, extending over an area of more than 200 square miles, and of an average depth of several feet. This turned out to consist chiefly of nitrate of soda, and it could be brought to England at a cost of less than one-half of that of the ordinary saltpetre or nitrate of potash. Many thousands of tons of this material are now annually shipped from the South Peruvian ports, and the greater part of this finds its way into the nitre-pots of the English sulphuric acid works.

The existence of this comparatively new source of nitre has given a fresh impulse to the manufacture of sulphuric acid, rendering it independent of the high and varying price of nitrate of potash.

Before entering the chambers, the mixture of sulphur dioxide, nitrous fumes, and air passes up a tower about forty-five feet high, having an outside covering of strong sheet lead, but lined inside with fire-brick, and having about fifteen feet of its lower part filled up with pieces of flint. This tower is closed at top and bottom, and the gases passing in by a side flue near the bottom, issue by another side-flue near the top of the tower. Above the closed top of the tower are placed two reservoirs made of lead. One of these contains strong sulphuric acid, which has been saturated with nitrous fumes at a subsequent part of the process, whilst the other is filled with a dilute- or chamber-acid, as it is termed, because it is that which is drawn off the floor of the chambers. By a very ingenious contrivance, given volumes of the strong nitrated acid and the weak chamber-acid are allowed to flow separately from these reservoirs down the tower. Here the two acids meet, and the strong acid, being thus diluted, gives up the nitrous fumes which it formerly held in solution, and these are carried away with the current of gases into the chamber. The downward

flow of acid acts in other respects beneficially. It cools the gases, which come almost red-hot from the pyrites burners, and would melt the lead of the chambers if not cooled. In this way a large quantity of water contained in the dilute acid is evaporated, and goes into the chamber in the form of steam, whilst the acid which falls to the bottom of the tower has become concentrated, and therefore more valuable. These denitrating towers are frequently termed "Glover's towers," from the name of their inventor; their use constitutes a very great step in the manufacture, but to be effective they must be employed in conjunction with another tower placed at the end of the last chamber, and called the Gay-Lussac tower, the mode of working of which I will explain to you shortly.

Entering the first chamber (for usually two or three chambers are worked in succession) at a height of eight, ten, or even fifteen feet from the floor, and having a temperature of about 75°, the mixed gases meet with jets of steam introduced at different parts of the chambers. In this first chamber the largest quantity of sulphuric acid is deposited, falling in drops on to the floor of the chamber, where it collects. In order to test the working of the chambers, a tray of lead, having an area of one square foot, and shown at (*f*), Fig. 5, is placed in a slanting position inside, but near one of the sides of the chamber, and a pipe is led from this to the outside. The acid as it falls on this tray runs out by the pipe, and thus the yield of one square foot of the chamber is measured. Several of these gauges, placed in different parts of the chamber, give the average production. Passing through the first chamber, which is often 90 or 100 feet long by 20 to 25 feet in breadth and 16 to 17 feet in height, and has therefore a capacity of somewhere about 40,000 cubic feet, the gases, which have by no means deposited all their acid, pass through a wide flue into the second chamber, where they again meet with steam, and having traversed this, pass again into the third, or exhaust, chamber, of the same size as the other two. Here, if the process is properly worked, all the sulphur dioxide is converted into sulphuric acid, so that the gases issuing from this chamber ought to contain no sulphur dioxide, but only air, aqueous vapour, and red nitrous fumes. In order now to prevent the escape of these

noxious and acrid red fumes into the air, and to enable the manufacturer to use them again, the Gay-Lussac tower is employed. This is built at the end of the third chamber, and is placed in contact with this on the one hand and with the chimney on the other, so that all acid fumes, before they can be discharged into the air by the chimney, must pass through the Gay-Lussac. It consists, like the Glover, of a square tower, fifty feet high, made of strong lead, fixed of course, on to a firm wooden frame, lined for thirty-five feet with glazed fire tiles two inches thick, and filled with hard coke. The exit gases from the third chamber are drawn in at the bottom of this coke column, and escape to the chimney by a flue at the top of the column. At the top of this tower there is a reservoir filled with strong acid, of specific gravity 1·75, obtained by concentrating the chamber acid. By means of an arrangement identical to that in the Glover's tower, a constant stream of this strong acid is allowed to flow down the tower, and thus trickling over the coke, meets the exhaust gases. The strong acid at once absorbs the nitrous fumes, which would otherwise pass up the chimney, and having become thus saturated with nitrous fumes, runs away into a reservoir, whence this nitrated acid is pumped up to the reservoir on the top of the Glover's tower, for use, there, in the way I have already described. The saving in nitre which this procedure enables the manufacturer to make is very large. In works where the Glover and Gay-Lussac towers are not used no less than from twelve to fourteen parts of Chili saltpetre are needed for every hundred parts of sulphur burnt, whereas when these means are employed the percentage of nitre is reduced to below five. Not only is this economy to the manufacturer, but it is a great benefit to his neighbour, for if properly managed scarcely a trace of either nitrous or sulphurous fumes ought to escape, and I shall be glad to see the day arrive when sulphuric acid works, without exception, are required to adopt these precautions.

The question naturally suggests itself to our minds as we study this subject, What do we know of the interior working of the chambers? What size of chamber is most efficacious? In what part of the chamber does most of the acid condense? What is the temperature of the gases which conduces to the largest yield? To these and many

more similar questions I am afraid we must at present be content with very general and unsatisfactory replies. One authority, the late Mr. Henry A. Smith, tells us that almost all the sulphuric acid is formed within three feet of the bottom of the chamber, whereas the very elaborate experiments of Mr. H. W. Deacon of Widnes seem to prove the exact opposite, inasmuch as he shows that no less than 79 per cent. of the total make of the chamber is yielded by the upper three or four feet out of a total height of twenty feet, the chamber being eighty-seven feet long and thirty feet wide. Then again the size of chamber best adapted for the manufacture is as yet undecided, as also. whether one, two, or three chambers should be worked in series. Again, the question of temperature is one about which the opinions of manufacturers differ, some believing that the higher the temperature of the gases is kept, the greater is the yield of acid, the only limiting condition being the solvent action of the gases on the lead, whilst others affirm that the yield of acid is always greater in winter than in summer, and that therefore the cooler you keep your chambers the more acid will you get.

From what I have said you will see that it is not possible to carry out the continuous process of acid making in the chambers beyond the point at which the acid attains a strength of specific gravity 1·55. When this strength is reached the acid is allowed to flow away from the chamber. But this acid is not strong enough for most of the purposes for which sulphuric acid is required. In order to obtain a stronger acid, either the arrangement of the Glover's towers, as I have described, must be used, or the chamber-acid must be concentrated in leaden pans over or under which a flame and heated air from a furnace play. The acid being much less volatile than the water, this latter passes away in the form of steam, and the strong acid remains. By this means the acid can be concentrated until it attains a specific gravity of 1·72, or contains 78 per cent. of the real acid. Beyond this degree of concentration the hot acid begins rapidly to attack the lead of the pans, and it therefore cannot be further evaporated. Even then all concentrated sulphuric acid contains lead in solution, as I can easily show you by pouring some of this clear transparent strong acid into a glass of water; you see

that the dilute acid which I have thus prepared is now turbid, and this is caused by the separation of insoluble sulphate of lead, which was held in solution, and was therefore invisible in the strong acid.

The hot acid is then run off from the concentrating pans into a leaden trough surrounded by cold water, whence it passes into reservoirs or carboys. In this form the acid is technically known as B.O.V., or brown oil of vitriol, as it is always slightly coloured from the presence of traces of organic matter. In this condition it is very largely sold for a great variety of purposes.

In order to concentrate the acid still further, and to drive off all the water which it contains, the concentrated oil of vitriol must be heated in vessels of either glass or platinum, substances which are not attacked by hot sulphuric acid of any degree of strength. In England glass vessels are most usually employed, whilst on the Continent platinum rectifying plant is more common. The glass vessels or retorts in which the acid is rectified are large, well-annealed, and evenly-blown vessels, capable of holding twenty gallons of the acid. Each retort is placed on an iron sand-bath, under which is a fire, so arranged, however, that the flame does not touch the retort. The acid having been heated in these retorts until all the water is driven off, is allowed to cool, and then drawn off into carboys. This is termed rectified acid. A very beautiful and quite novel arrangement for rectifying oil of vitriol in platinum is exhibited by Messrs. Johnson, Matthey, & Co. By means of this arrangement all evaporation of the acid in leaden pans is avoided, and thus the operation is not only cheapened, but the acid obtained is nearly free from lead. The new arrangement consists of two pans made of corrugated plates of platinum, and heated by a fire; at one end of these the chamber acid is allowed to run in a thin stream, whilst at the other a continuous stream of concentrated acid is obtained. For the purpose of completing the rectification the acid flows into a retort also of platinum having a corrugated surface, and then the perfectly strong acid runs out through a platinum worm into the glass carboy in which it is sent to market.

We may next ask, Is the usual working of the chambers in England as good as it can be? In answer to this I may reply that as a general rule a yield of 290 parts of pure

acid from 100 of sulphur is practically considered about the proper production, so that, taking the theoretical yield to be 306 parts, the average loss would be about five per cent. of sulphur. In cases where special precautions are taken, the yield reaches 297 parts, but not unfrequently, I am afraid, much less careful working is observed, and large escapes of sulphurous fumes then occur. Thus in his Eighth Annual Report (1871) Dr. R. Angus Smith, the well-known chief government inspector of alkali-works, gives (p. 17) a table, showing the total escape of sulphur acids (calculated as sulphuric acid) from twenty-three chemical works. From this it appears that whilst from some of the works no escape of these acids occurs, the average loss of sulphur in the twenty-three works in question is 7·606 per cent. on the total quantity burnt, and that the loss in the case of four works actually rises to more than 20 per cent., in one case amounting to an escape of 159 lbs. of sulphuric acid every hour. "Facts like these," says the inspector, "dispose of the argument often used by the manufacturers, that they require the acid, and that it is to their interest to keep it, and of course condense it to the best of their power. Indeed certain makers are fully aware that they are allowing sulphuric acid to escape in large quantities, but their reply is that it is cheaper to permit a large escape, and work rapidly, rather than have large chambers, and condense the whole of their gases." Hence there can be no two opinions as to the desirableness of compelling such makers to work properly, and an extension of the Alkali Act (which now, so far as a definite limit is concerned, only applies to hydrochloric acid) to the sulphur acids seems to be imperatively called for in the interests of the nation.

Through the kindness of several eminent manufacturers I am able to give a practical test of the performance of the process in different works in England and on the Continent. Thus Messrs. Gaskell, Deacon, and Co. of Widnes find as a result of eighteen months' work, including stoppages, &c., during that time, that one cubic metre of chamber space takes 5·74 kilos of sulphur per week, producing 15·98 kilos of strong sulphuric acid (H_2SO_4). It is to be remembered, however, that if the working is measured only for one day, when all is in good order, this number would be materially raised. At the works of Messrs. Muspratt

at Liverpool one cubic metre of chamber uses 5·25 kilos of sulphur per week, and at a second works of the same firm at Widnes, 6·3 kilos are used for the same space, and a production of 290 parts of strong acid for every 100 of sulphur burnt is their average. On the Tyne the numbers are similar, and at the Washington Works, on the Wear, the average of ten years' work shows that 100 parts of sulphur yielded 292 parts of strong acid, whilst the average quantity of nitrate of soda needed is 5·5 for every 100 of sulphur received.

In the Rheinau Works, near Mannheim, in Germany, one cubic metre chamber space takes 3·88 kilos of sulphur per week, giving 11·40 kilos of H_2SO_4, so that 100 of sulphur yield 294 of strong acid, and 6·5 parts by weight of nitrate of soda and 210 parts by weight of steam, are needed to complete the decomposition.

In order to convert 100 parts of sulphur into sulphuric acid, about 210 parts of water in the form of steam are needed. This steam is costly in its production, and Dr. Sprengel has recently proposed to reduce this item of expenditure by employing a jet of water in the form of spray, or in a state of very minute division.

None of these processes, you will see, yield us chemically pure acid. For this purpose the acid must be distilled to get rid of the arsenic, lead, and other non-volatile matter which the rectified acid contains. Even then the acid is not perfectly pure, as it contains water. This cannot be got rid of by distillation, but if we cool the distilled liquid, the pure acid, containing 100 per cent. of H_2SO_4, can be crystallised out.

In some works, both here and on the Continent, sulphuric acid made from pyrites is freed from the arsenic contained in it by being brought into contact with sulphuretted hydrogen. The arsenic is separated as the trisulphide which settles out from the acid. To effect this the sulphuric acid, before concentration, is made to flow in divided streams down a tower of lead five feet square and fifteen feet high, the fall of the acid being retarded by a large number of bars of wood covered with lead, which stretch horizontally across the tower. A current of sulphuretted hydrogen gas, generated, as in the laboratory, by the action of sulphuric acid on ferrous sulphide, passes up the tower, and thus meets the acid in its descent.

From what I have said, you will understand that in a manufacture like that of sulphuric acid, where enormous volumes of acid and deleterious gases have to be dealt with, it is only by the careful application of scientific principles that the process can be economically carried on, and carried on so as not to be a nuisance to the neighbourhood. In all the works which are scientifically conducted we not only find the Glover and Gay-Lussac towers in proper action, but we observe that the manager keeps a daily record of the work of his chambers; he ascertains moreover the proportion of air and sulphur dioxide which enter his first chamber, and the ratio of sulphur dioxide and nitrous fumes which leave his last chamber, and thus he keeps a complete hold over his process, and can tell at any moment when anything goes wrong. The scientific vitriol manufacturer too is a blessing instead of a curse to the district in which his works are situated. The escape of noxious vapours from a properly-managed sulphuric acid plant is inappreciable, whilst the manufacture creates a demand for labour, and becomes a source of income to hundreds of families. All the more reason is there, then, that careless or ignorant persons engaged in this trade should be compelled to adopt the best and most perfect scientific methods of manufacture, and that the wasteful escapes of noxious vapours, even too common at the present day, should be altogether prevented.

LECTURE II.

ON THE ALKALI MANUFACTURE.

THE history of the manufacture of carbonate of soda, or soda-ash, from common salt is one of peculiar interest, and I therefore need make no apology for bringing before you the chief facts respecting the origin and growth of this important branch of chemical industry.

Previous to the year 1793 the whole of the carbonate of soda of commerce was obtained from the ashes of sea-plants or kelp, collected on the north-west coasts of Spain, France, Ireland, and Scotland. The quantity however of alkali thus obtained from sea-plants was much less than that which came to Western Europe from Russia and America in the form of potashes, the characteristic alkali of land-plants.

One of the first effects of the French Revolution was to cut off this supply of alkali from France, and therefore to diminish many important manufactures, such as the soap trade, which are dependent upon its use. Under these circumstances the Government of the day issued an appeal to the French chemists urging the importance of obtaining all the materials deposited in their own country by nature, "so as to render vain the efforts and hatred of despots;" and commanding all citizens who "have commenced establishments or who have obtained patents for the manufacture of soda from common salt, to make known to the Convention the locality of these establishments, the quantity of soda supplied by them, and the quantity they can hereafter supply." A commission was appointed to investigate this subject; and in 1794 they reported on thirteen different processes for the manufacture of soda-ash from common salt, the particulars of which had been submitted to them. The preference was given to the operations devised by an apothecary of the name of Leblanc, who had already erected a soda manufactory near Paris, which had been at work some time previously.

The report gives a full description of the various processes which constituted Leblanc's invention; these consisted of

(1) The decomposition of the common salt (NaCl) by means of sulphuric acid (H_2SO_4), and the consequent production of sulphate of soda (Na_2SO_4), with evolution of hydrochloric acid gas (HCl);

(2) The decomposition of the sulphate of soda or salt-cake (Na_2SO_4), by means of chalk and coal, and the consequent production of black-ash, consisting essentially of a mixture of soluble carbonate of soda (Na_2CO_3), and insoluble calcium monosulphide (CaS);

(3) The separation of the constituents of the last product by lixiviation with water and the preparation of the soluble carbonate of soda.

This process elaborated by Leblanc before the time of the French Revolution is in fact that which is now employed in all the alkali-works in the world without having undergone any material alteration. The commissioners say in their report:—"Citizens Leblanc, Dizé, and Shée were the first who submitted to us particulars of their process, and this was done with a noble devotion to the public good. Their establishment had been formed some time previously at Franciade; but the consequences of the French Revolution and of the war which followed having deprived them of funds, the works were suspended, and for some months past the manufactory has become a national establishment and was successfully at work in the year 1794." The operations however did not proceed satisfactorily, the quantity of soda turned out was smaller than had been expected, the operations were discontinued, and Leblanc and his partners applied for and received assistance from the English Government. It is sad to have to relate that the man who thus originated a world-wide industry did not benefit from his discoveries, but died in a French asylum for paupers.

The establishment of numerous alkali-works in France was the natural result of this discovery; many of these were situated at Marseilles, the seat of the French soap trade, and conveniently placed for obtaining three of the necessary raw materials: (1) Sulphur, imported from Sicily; (2) salt, obtained by the evaporation of salt-water by the sun's heat; (3) limestone. It had however the

disadvantage of being at a distance from the fourth necessary raw material, viz., coal.

Although the process for making alkali was published in the *Annales de Chimie* for the year 1797, it is remarkable that some years elapsed before this process was taken up in England. This may be partly accounted for by the fact that as war was then raging, communication between the two countries was almost entirely cut off, but perhaps especially because of the high war-duty on salt which existed up to the year 1823.

In the year 1819 Mr. Charles Tennant, of Glasgow, erected sulphuric acid chambers in that city for the purpose of decomposing salt and manufacturing soda on Leblanc's process, and in the year 1821 Mr. Losh, of Newcastle, erected works at Walker on the Tyne for the manufacture of soda crystals, which were then obtained from kelp and sold at 60*l.* per ton. Mr. Losh made use of a weak brine-spring for the purpose of obtaining his salt, having been permitted to employ this free from salt-duty, but the extent of his works was naturally limited by the use of this brine instead of the solid salt. At this time the price of sulphur was 7*l.* per ton, the duty being remitted; and the cost of nitrate of potash was 32*l.* per ton; the continuous sulphuric acid process, which, as I mentioned in my previous lecture, had been devised by Chaptal, being at this time employed.

The year 1823 may really be considered the one in which the alkali trade began, inasmuch as in that year, the duty having been taken off salt, Mr. James Muspratt commenced the erection of works at Liverpool, at once adopting Leblanc's process.

Another discovery of enormous commercial importance was that of the bleaching action of chlorine by Scheele in 1774, and the subsequent application by Berthollet of the bleaching action of the hypochlorites of potash and lime. In 1787 Professor Copeland of Aberdeen brought over Berthollet's process to Scotland, and in concert with the Duke of Gordon commenced works in Aberdeen for the manufacture of chlorine in large Woulff's bottles, and applied the process successfully to bleaching calicoes.

Up to this time the bleaching of all the cotton goods made in Lancashire was effected by exposure to sunlight

and air, nearly all the cloth being sent to Holland or Germany for this purpose, where it remained for the summer, and was brought back for sale in the winter. In the year 1789 a large establishment for bleaching by chlorine was erected near Bolton in Lancashire, and lime was very soon employed in place of Berthollet's hypochlorite of potash, or *eau de Javelles;* and in 1799 Mr. Tennant obtained a patent for the manufacture of chloride of lime in dry powder. In 1798 an Act was passed to permit a drawback of the duty on salt consumed in making hydrochloric acid necessary for the production of chlorine for bleaching purposes. It was required, however, that the residue of sulphate of soda should be thrown away, thus effectually preventing its application to the manufacture of soda; this restriction was not done away with till the year 1814. It is singular to find that, although the hydrochloric acid evolved in the first part of Leblanc's process was needed for, and had been already applied to the manufacture of chloride of lime, the early alkali-makers did not see the necessity of incorporating the manufacture of this bleaching compound with that of their alkali, but on the contrary allowed the whole of their hydrochloric acid to pass into the chimney without any attempt at condensation.

The quantity of this acid gas which was evolved from Mr. Muspratt's chimneys at Liverpool became so large that he was proceeded against by the corporation of that town as causing a nuisance; and he was compelled to remove his works from Liverpool to Newton.

The effectual remedy for this evil was found by Mr. William Gossage in the year 1836. He passed the hydrochloric acid gas up condensing towers, filled with coke or broken bricks, down which a current of water passed. By this means a perfect condensation of hydrochloric acid is effected; for, as you are all aware, this gas dissolves in water in very large quantities, and so completely is this solution effected that the escaping gases when passed through nitrate of silver frequently show no trace of hydrochloric acid. So completely can this be carried out that an Act was passed in the year 1863 compelling manufacturers to condense within 5 per cent. of the total amount of hydrochloric acid they evolve; whilst the second Act,

of 1874, further declared that no alkali-works shall emit hydrochloric acid gas of such strength as to contain more than 0·2 grain (one-fifth of a grain) in every cubic foot. So that the alkali-maker is now compelled not only to condense 95 per cent. at least of all the hydrochloric acid he makes, but he is not allowed to emit that 5 per cent. unless it shall be so diluted with air or other harmless gases that

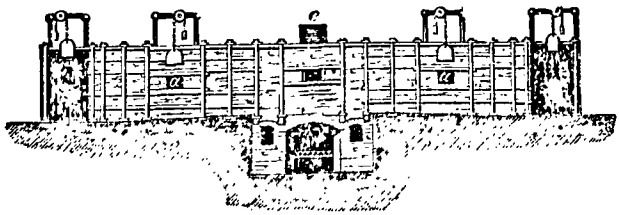


FIG. 8.

one cubic foot of the emanating gas contains less than one fifth of a grain of hydrochloric acid.

We will now proceed to consider the various parts of Leblanc's process, and discuss the chemical changes which here occur.

1. *The Salt-cake Process.*—This process is usually commenced in large cast-iron pots, and completed in reverberatory furnaces or roasters. Figs. 8 and 9 show the con-

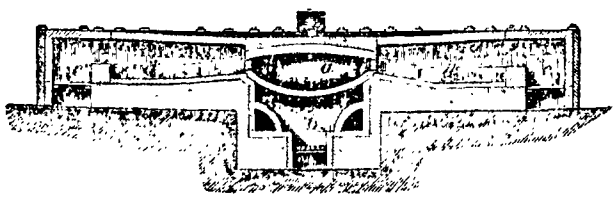


FIG. 9.

struction of one of the forms of salt-cake furnace in use. It consists of the large covered iron pan (*a*) Fig. 9 placed in the centre of the furnace and heated by a fire placed underneath, and two roasters (*dd*) placed one at each end, on the hearths of which the salt is completely decomposed.

The charge consists of 16 cwt. of common salt, which is

placed in the iron pan, and on to this is run the quantity of sulphuric acid necessary completely to decompose it. This amounts to 123·5 gallons, or 1800 lbs. of chamber-acid having a specific gravity of 1·42. Torrents of hydrochloric acid are then given off, the decomposition which takes place being represented by the equation—

$$H_2SO_4 + NaCl = HNaSO_4 + HCl.$$

This process lasts about one hour, and the temperature

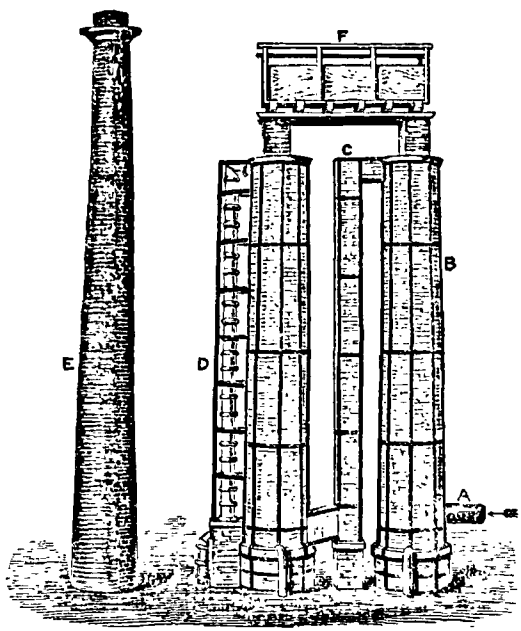


FIG. 10.

of the mass rises to about 120° Fahr. All the hydrochloric acid which is thus evolved passes directly from the pan by means of the flue (*e*) Fig. 8 into the hydrochloric acid condensing towers (Fig. 10). These towers are often 50 or 60 feet in height, and are usually built of Yorkshire flag clamped together with iron, the joints being rendered gas-tight by a cord of vulcanized rubber.

The acid vapours enter the tower (B) at (A); and in passing up this tower, which is either filled with piled bricks or with hard coke, they meet with a descending current of water supplied from the cistern (F). The strongly acid liquors flow away by a pipe at the bottom of the tower (B), and are stored by the manufacturer for subsequent use. Any hydrochloric acid vapours unabsorbed in the tower (B) pass down the brick tunnel (C) into the second tower, which they again ascend and meet another current of falling water. When the vapours reach the top of this tower they ought to be perfectly free from hydrochloric acid gas, and are then allowed to pass away into the chimney (E).

After the mixture has been heated for about an hour in the salt-cake pan and has become solid, it is raked through the doors (*a a*) Fig. 8 on to the hearth of one of the furnaces or roasters at each side of the decomposing pan. Here the hot air and flame from the fire at the end complete the decomposition into sodium sulphate and hydrochloric acid, as expressed by the following equation :—

$$NaHSO_4 + NaCl = Na_2SO_4 + HCl.$$

The acid vapours from the pans usually go into condensers by themselves, those from the roasters or salt-cake furnaces going into other condensers. The acid liquor obtained in the latter case is weak, because large quantities of water are needed to cool the heated air which passes along with the hydrochloric acid gas into the towers. As soon as the decomposition is complete the salt-cake (Na_2SO_4) is withdrawn from the furnace and kept for the subsequent process. Ten of the above charges are usually drawn in one day, so that eight tons of salt and about the same weight of oil of vitriol are used, whilst rather less than five tons of gaseous hydrochloric acid and nearly twenty tons of salt-cake are formed.

Here you have a complete analysis of commercial salt-cake, from which you will see that it contains about 3·5 per cent. of impurity.

COMPOSITION OF SALT-CAKE.

Sodium sulphate, Na_2SO_4	96·150
Calcium sulphate, $CaSO_4$	0·923
Sulphuric acid, H_2SO_4	0·616
Sodium chloride, NaCl	1·345
Ferric oxide, Fe_2O_3	0·191
Water, H_2O	0·187
Insoluble matters	0·130
Loss	0·093
	100·000

The furnaces such as I have described are termed *open* roasters, in opposition to the second kind of salt-cake furnace to which the name of *close* roasters is given. In the first kind the hot air and flames from the fire pass over the salt-cake, and the products of combustion pass along with the acid vapours into the condenser. Hence by this mode of working, a part of the condensed acid is rendered both weak and impure, and much annoyance is caused by the condensers becoming choked with soot and dust from the fires. For this reason the second method is employed in many works. The pan is built at the side of the roaster instead of being placed in the centre, and the acid from the first part of the decomposition being concentrated and unmixed with air, passes into a condenser where a saturated or fuming aqueous hydrochloric acid is prepared, whilst the gases from the roaster are separated from the products of combustion by enclosing the hearth of the furnace by a fire-brick arch, between which and the top of the furnace the flames and hot air from the fire pass. So that the salt-cake on the hearth is placed in a kind of brick chamber or muffle, being heated from the hearth under which the fire passes and by radiation from the hot arch. Thus no soot or dirt from the fire can be carried into the condensers, and these do not become clogged or choked, and thus a perfect condensation is rendered possible.

Unfortunately these advantages are not wholly unaccompanied by drawbacks, which if not important for the manufacturer, are at least serious to his neighbours. The arch separating the roaster-hearth from the fire gases cannot in practice be kept gas-tight. It is continually cracking from

unequal expansion, and as soon as a crack occurs, the hydrochloric acid gas is pulled or drawn by the draft of the chimney through that crack, and then passes up the chimney, where the pressure is somewhat less than that of the atmosphere, and is delivered into the air, rather than through the condensers, where the pressure is somewhat greater than the atmosphere. This is a fertile source of annoyance in the neighbourhood of alkali-works, and one which it is difficult

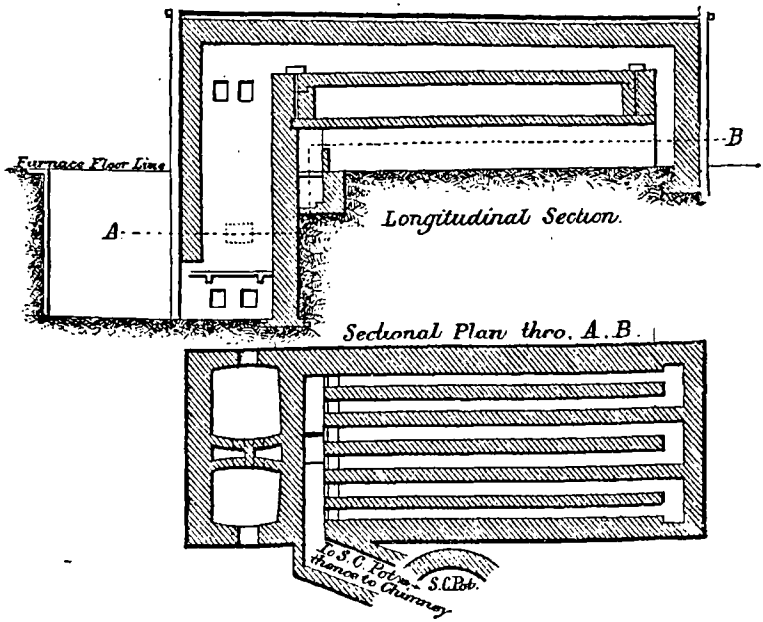


FIG. 11.

for the inspector to calculate for. Several suggestions have been made with the view of obviating this difficulty in the use of the closed roasters.

One of the most feasible and ingenious of the proposed schemes is that patented by the late Mr. Deacon, of Widnes. You see in the drawing that the fireplace is built contiguous to, but several feet below the brick chamber or muffle in which the acid gas is evolved. It follows from this

difference of level that there is a column of heated gases several feet in height immediately over the fire-bars, and the upward movement of this heated and rarefied air receives a check in going round the muffle, so that the ordinary state of things is reversed, and the pressure in the flue round the muffle is greater than that in the interior of the muffle, thus altogether preventing any chance of escape of the acid vapours from the muffle to the chimney, though allowing some of the gaseous products of combustion to pass through the unavoidable cracks into the interior of the muffle, but in such small quantity that they do not interfere at all with the working of the process. I have little doubt that before long either this or some similar salt-cake furnace will come into general use.

We owe another valuable and quite recent improvement in the salt-cake process to Messrs. Jones and Walsh of Middlesborough. This consists in a mechanical arrangement by which all hand labour is dispensed with, and by which the whole operation, from the mixing of the materials to the production of the finished dry salt-cake, is carried on in one large pan.

A third proposal made by Messrs. Cammack and Walker seems to me to be based upon a more scientific view of the decomposition than any of the former plans. When large masses of salt and sulphuric acid are brought together, the reaction—as you see when I pour this bottle full of acid on to the salt contained in this flask—is at first very violent, and torrents of hydrochloric acid gas are evolved. The action however soon moderates, as you notice. During the first twenty minutes the main quantity of acid has come off from the salt-cake pan, and during the remaining three hours needed to complete the reaction, probably only a small quantity of gas enters the condensers. So that for twenty minutes the condensers have more work than they can properly do, whilst after that time they are underworked. Messrs. Cammack and Walker's plan seeks to obviate these inconveniences of all the old processes by sending in at one end of the heated space in which the reaction occurs a constant stream of salt and acid mixed in the right proportions, and drawing off at the other end the finished salt-cake. Thus a never increasing and never decreasing stream of hydrochloric acid gas is sent into the condensers, and a constant supply of the solid product is furnished. In this way the

reaction can be most completely kept under control, and all irregularities and, therefore, chances of escape of acid vapours rigidly prevented. How far this proposal can be practically carried out remains I believe yet to be ascertained, but I am sure we must all wish success to the ingenious inventors of what I may truly say is the most scientifically correct proposal yet made for the manufacture of salt-cake from sulphuric acid and common salt.

I daresay you may be interested to learn the principles upon which the Government Inspectors of alkali-works determine the question as to whether a given manufacturer of salt-cake is complying with the Acts passed for the prevention of the pollution of the air, and the destruction to property which takes place when hydrochloric acid gas is not properly condensed. I can, however, only indicate the methods adopted, and for particulars on this, and many kindred subjects of great interest, I must refer you to the valuable series of annual reports issued by Dr. R. Angus Smith.

The inspector has to ascertain whether the manufacturer is working in compliance with the Acts—that is (1) whether he is sending out less than 5 per cent. of his total make of hydrochloric acid; and (2) whether the gaseous products passing out into the air contain more than one-fifth of a grain of hydrochloric acid per cubic foot. For the purpose of settling the first of these points, he may determine the proportion between the amount of acid gas contained in one cubic foot of the gases passing *into* the condenser, and that contained in the same bulk of gases *leaving* the condenser, and thus get to know how many parts of acid escape for every 100 which are made. The inspector may also ascertain the absolute amount of hydrochloric acid gas which passes up the chimney, and then compare this with the total quantity which the manufacturer evolves, calculated from the total weight of salt which he decomposes.

The method by which the quantity of hydrochloric acid contained in a cubic foot of chimney gases can be ascertained is very simple. We only need to draw a slow current of the gases by means of an aspirator through three bottles containing water, placed one after the other. As soon as one cubic foot of water has flowed out of the aspirator, we know that one cubic foot of chimney gases has passed through our bottles. The whole of the hydro-

chloric acid contained in this volume has been absorbed by the water, and we now only need to precipitate with silver nitrate and estimate the chloride of silver in order to learn how much acid there is in each cubic foot of chimney gases. This, multiplied by the number of cubic feet passing up the chimney in any given time, as ascertained by the very ingenious anemometer devised by Mr. A. E. Fletcher, one of the inspectors under the Act, gives the total amount of escaping hydrochloric acid in that particular time.

Before leaving the salt-cake process, I must refer to a new and very interesting mode of preparing this substance by a process differing from that of Leblanc, which from the name of the inventor is termed the Hargreaves process. The object of this is to manufacture salt-cake directly from salt, sulphur dioxide (sulphurous acid) and water, so as altogether to do away with the manufacture of sulphuric acid. It depends upon the principle, that although sulphurous acid cannot by itself decompose salt, it is able to do so in the presence of air, or rather of oxygen, and of vapour of water, if time enough be allowed.

In order to effect this decomposition, a series of large kilns or stoves are built of brick, so arranged that each kiln can be put into communication with its neighbour, and each heated by a fire. Each kiln is then filled with dried and porous salt (NaCl), and the gases from the pyrites burners led directly into these kilns one after the other. By careful attention to temperature and to the quantity of air and steam admitted with the sulphur dioxide, it is possible in this way to decompose the salt as completely into sulphate of soda (salt-cake) as when heated by the old process with sulphuric acid. The chemical reaction, I need scarcely say, is just the same in both cases, thus :—

$$2\,NaCl + SO_2 + O + H_2O = Na_2SO_4 + 2HCl.$$

Hydrochloric acid gas is given off from the Hargreaves kilns as from the ordinary salt-cake pan or roaster, and must in like manner be passed into the condensers.

2. *The Black-ash Process.*—I must now ask you to accompany me through the second portion of Leblanc's system, the so-called Black-ash process.

The theory of this process is a simple one, so far as the chief products are concerned, but it is complicated

when we come to consider the mode of formation of the many distinct compounds which make their appearance in the course of the reaction. I can here only attempt to give an explanation of the chief features of the case.

The first chemical change which the salt-cake (Na_2SO_4) undergoes in its passage to carbonate of soda is the reduction to sodium sulphide (Na_2S), by heating it with slack, or powdered coal; thus :—

$$Na_2SO_4 + C_4 = Na_2S + 4CO.$$

The second change which occurs is the conversion of the sodium sulphide into sodium carbonate (carbonate of soda), by heating it with chalk or limestone (calcium carbonate). The reaction which then takes place is represented by the equation—

$$Na_2S + CaCO_3 = Na_2CO_3 + CaS.$$

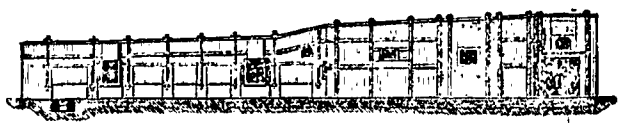


FIG. 12.

In practice these two reactions are carried on at the same time, a mixture of about 15 parts by weight of salt-cake, 16 parts of limestone, and 6 of coal, being heated in a reverberatory furnace, termed a balling-furnace, an elevation and section of which you see in Figs. 12 and 13. After exposure to the reducing flame of this furnace for two hours the charge consisting of $4\frac{3}{4}$ cwt. is fluxed and fully decomposed, and then the liquid mass is scraped out into

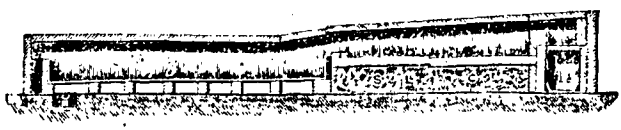


FIG. 13.

iron barrows or trucks and allowed to cool, and in this state is known as black-ash ball—so called from the colour of the mass.

In place of the old black-ash furnace or balling-furnace in which the reaction is completed by hand-labour, a new furnace, termed a revolving black-ash furnace, is being now largely employed. A representation of such a revolving furnace is seen in Fig. 14. In this the mixing of the materials is effected mechanically. The charge, usually consisting of 30 cwt. of salt-cake, 32 of limestone, and 20 of slack, is introduced by means of a hopper into the large cylinder, B, placed horizontally. Through the axis of this cylinder, B, flames from a furnace, A, Fig. 14, are allowed to pass.

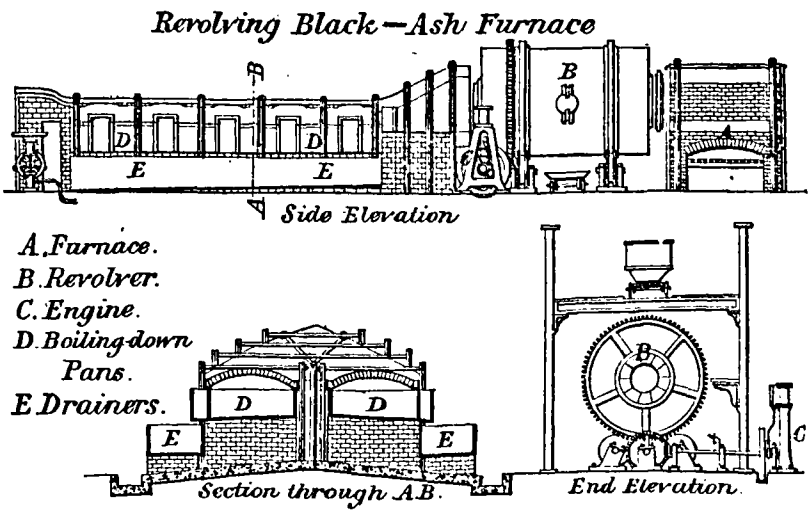


FIG. 14.

This cylinder revolves first at a slow rate, and afterwards with an increasing velocity, finally reaching a maximum of five or six revolutions per minute. The cylinder is 10 to 12 feet in diameter and 15 to 18 feet long. Each charge takes about two hours to work off, and when completed the door seen in the side of the cylinder is opened and the fused mass allowed to flow into the iron trucks placed beneath it. It yields 10 balls, each being a small truckfull of black-ash, weighing 3 cwt.

The advantages which the revolving black-ash furnace has over the hand-worked ones chiefly consist in the saving

of labour and in the production of a material which possesses a more constant composition.

The following Table gives the charges, theoretical and practical, of salt-cake, coal, and limestone used in various works :—

BLACK-ASH CHARGES.

Theoretical, $\underset{142}{Na_2SO_4} + \underset{100}{CaCO_3} + \underset{48}{C_4}$.

	Lbs. practice.	Lbs. theory.
No. 1 Works—		
Salt-cake	150	150
Limestone	160	105·63
Coal-dust	60	50·70
No. 2 Works—		
Salt-cake	336	336
Limestone	336	236·62
Coal-dust	203	113·57
No. 3 Works—		
Salt-cake	252	252
Limestone	294	177·46
Coal-dust	161	85·18
No. 4, Lancashire charge—		
Salt-cake	224	224
Limestone	224	157·74
Coal-dust	140	75·71
No. 5, Tyne charge—		
Salt-cake	196	196
Limestone	252	138·02
Coal-dust	126	66·25

Here you have a table of analyses of black-ash. You see that it contains a large number of other salts besides carbonate of soda and mono-sulphide of calcium, though these two constitute its main ingredients. In practice a large excess of lime is used, and this gives rise, as we shall see, to caustic soda in the black-ash.

COMPOSITION OF BLACK-ASH.

1 and 2, German Ash, analysed by Unger and Stohmann.
3, English Ash, analysed by Brown and Kynaston.

Constituents.	1	2	3
Sodium sulphate	1·99	1·54	0·395
Sodium sulphide	—	—	—
Sodium chloride	2·54	1·42	2·528
Sodium carbonate	23·57	44·41	36·879
Sodium silicate	—	—	1·182
Caustic soda ; hydrated..	11·12	—	—
Sodium aluminate... ..	—	—	0·689
Calcium carbonate ...	12·90	3·20	3·315
Calcium sulphide	27·61	30·96	28·681
Calcium sulphite	—	—	2·178
Lime	7·15	8·35	9·270
Magnesia	—	0·10	0·254
Magnesium silicate ...	4·74	—	—
Alumina	—	0·79	1·132
Water	2·10	—	0·219
Ferric oxide	—	1·75	2·658
Ferrous sulphide	2·45	—	0·371
Silica	—	0·89	—
Sand	2·02	2·20	0·901
Charcoal...	1·59	5·32	7·007
Ultramarin	—	—	0·959
Total	99·78	100·93	98·18

The next operation consists in the separation of the carbonate of soda from the insoluble calcium mono-sulphide, and the other impurities. This is easily effected by the process of *lixiviation*, or washing out the soluble carbonate of soda, leaving the mono-sulphide of calcium and the excess of lime and carbonate of lime as insoluble powders behind. In this process the object aimed at is to dissolve as large a quantity of the carbonate of soda with as small a quantity of water as possible. The arrangement of lixiviating vats for effecting this is the invention of the late Mr. Shanks of St. Helens. A series of vats is employed in which the broken black-ash is placed; pure water is allowed to flow on to the ash, which has already been nearly exhausted, and the solution then passes on until the nearly

saturated liquors come in contact with the fresh black-ash. In this way with the least quantity of water the complete lixiviation of the black-ash is effected. The average time needed for working off a vat is about forty-eight hours.

The residue remaining in the vats after the soluble matter has been extracted constitutes what is known as the alkali-maker's waste.

It is impossible, however, by this process of lixiviation completely to wash out the soluble carbonate of soda, so that we find about 3 per cent. of soluble alkali left in the vat waste.

I need scarcely remind you that this waste contains the whole of the sulphur from the salt-cake; and this as a rule is in the insoluble state of sulphide of calcium. This waste is now thrown away, being either made into heaps which often become a nuisance in the neighbourhood of the alkali-works, carried out to sea, or otherwise made away with.

A considerable excess of limestone is used in the manufacture of black-ash, and this in the course of the reaction becomes converted into caustic lime; on treating the mass with water this caustic lime transforms a considerable quantity of the carbonate of soda into caustic soda, and carbonate of lime is formed; thus:—

$$Na_2CO_3 + CaO + H_2O = 2NaOH + CaCO_3.$$

Hence it happens that as a rule about one-third of the total amount of soluble soda present in the black-ash liquor is caustic. Besides this, the black-ash liquor usually contains small quantities of sulphide as well as sulphocyanide of sodium.

In order to evaporate the black-ash liquors the waste heat of the black-ash furnace is employed. Large pans (D D, Fig. 14) are filled with the liquor, the water is boiled off, and the crystals which are deposited are scraped out into the drainers, E E, Fig. 14. These crystals are then heated, and yield common soda-ash, as it is termed. In order to separate the sulphide of sodium as well as the caustic soda which the black-ash liquors contain, these liquors are frequently oxidized and carbonated by the liquid being allowed to fall down towers, filled with coke, up which hot air mixed with carbonic acid passes.

Dr. C. R. A. Wright has made a series of experiments on the loss of soda occurring in the black-ash process. He believes the loss to be as follows:—

Sodium salt undecomposed...	3·49 per cent.
Sodium compounds rendered insoluble	5·44 ,,
Sodium compounds vaporized	1·14 ,,
	10·07 ,,

The next branch of the soda manufacture to which I must direct your attention is that of the preparation of soda crystals or washing soda ($Na_2CO_3 + 10H_2O$). For this purpose a warm saturated solution of carbonate of soda is allowed to stand for some weeks in large tanks. The crystals are gradually deposited and frequently grow to a very large size. The mother-liquor is then run off, and the mass of solid crystals broken up for market.

Bicarbonate of soda ($NaHCO_3$) is the next product. This is largely used in medicine, as also in the preparation of baking-powders, effervescing powders, and drinks. It is made by simply exposing crystalline masses of the ten-atom hydrate to the action of carbonic acid gas generated from limestone. This gas is greedily absorbed by the crystals, which lose their water of crystallization, and with it their transparency,

$$Na_2CO_3,\ 10H_2O + CO_2 = 2NaHCO_3 + 9H_2O.$$

The production of solid caustic soda has lately become an important branch of the alkali industry. I have already stated that the black-ash liquors contain a considerable quantity of caustic soda. When the black-ash crystals separate out, a mother-liquor, to which the name of red-liquor is given, is left behind, containing the whole of this caustic soda, and this is now largely used for the production of solid caustic alkali. For this purpose, however, the carbonate of soda, which is likewise contained in solution in the red-liquor, must be causticized, and this is done by reducing the strength of the liquor from specific gravity 1·28 to specific gravity 1·10, and then boiling this by means of steam with milk of lime, the reaction which occurs being:

$$Na_2CO_3 + Ca(OH)_2 = 2NaOH + CaCO_3.$$

After the carbonate of lime has separated out, the liquor is run off, the lime-mud remaining behind being well washed to extract the soluble caustic soda, and these washings being used for diluting fresh quantities of the red-liquor. The dilute caustic liquors are then either boiled down in ordinary boilers or evaporated in open pans, and afterwards concentrated in cast-iron pots sufficiently large to hold from eight to ten tons of finished caustic soda, being frequently 9 feet in diameter and $5\frac{1}{2}$ feet deep. These pans are built into brickwork and are heated by means of a furnace placed either below or on one side. At 120° C. the liquor in the pots begins to boil and ammonia gas is evolved; the temperature gradually rises until it reaches 260° C. The pots are next loosely covered, and the fire urged until the contents of the vessel are raised to a dull red-heat. It is now necessary to oxidize the small quantities of sulphides and cyanides which the red-liquors contain. This is effected by throwing in a few handfuls of nitre, or in some instances by pumping air through the hot liquid. When nitre is employed torrents of ammonia gas are given off, and a black deposit of graphite, first observed by Dr. Pauli, occurs. The white caustic soda contains 70 per cent. of Na_2O, or nearly 90 per cent. of the hydrate NaOH; a less concentrated product containing 60 per cent. of Na_2O is made, and this is termed cream caustic. When the operation is finished the molten mass is ladled out by means of copper ladles into iron drums, where it is allowed to cool; and thus preserved from the action of the air it may be transported to any distance. This of course is the best form of concentrated alkali, and caustic is therefore largely exported to districts where soap is needed, and where fats can be obtained in quantity but where carriage is a matter of consequence.

We next come to consider the alkali waste. For every ton of soda-ash produced, from one-and-a-half to two tons of waste is formed; and those of you who have been in the neighbourhood of alkali-works know in what enormous quantity this waste accumulates. This waste contains, as I have said, the whole of the sulphur burnt in the pyrites kilns, amounting to from 15 to 20 per cent. of the weight of the waste. The importance of recovering this sulphur, not only for the sake of the manufacturers them-

selves, to whom now it is entirely lost, but also for the sake of diminishing the nuisance to the neighbourhood, is patent to all. The waste, even when carefully stamped down and when the surface is made as hard as possible or is even covered with cinders, invariably undergoes a certain amount of oxidation. The insoluble mono-sulphide of calcium then becomes converted into a soluble higher sulphide, and this coming in contact with rain or drainage water dissolves, and the solution finds its way into the ordinary drains or streams of a district. There meeting with the carbonic acid of the air, or with the acid discharges from the chemical works, this yellow liquor evolves sulphuretted hydrogen sufficient to become a nuisance to the inhabitants of districts lying even several miles away from the waste.

Many proposals have been made for recovering the sulphur from the waste. Amongst these, that patented in England by Mr. Ludwig Mond is the most important, and has proved the most successful. This process depends upon the fact that the newly lixiviated waste, being porous and warm, undergoes oxidation when exposed to the action of air with formation of soluble sulphur compounds, so that on treating the oxidized waste with water not less than half of the total sulphur is obtained in solution in such a condition that, upon acidification with hydrochloric acid, the whole of the sulphur in combination is precipitated in the form of a finely-divided yellow powder, some fine samples of which you here see. The form in which the sulphur dissolves is chiefly as hyposulphite and disulphide of calcium, and these must be present in such proportions that on treatment with acid two molecules of sulphuretted hydrogen are evolved to one molecule of sulphurous acid; and hence sulphur alone is precipitated without evolution of either of the two gases; thus:—

$$2H_2S + SO_2 = S_3 + 2H_2O.$$

As much as 50 per cent. of the sulphur may, according to Mr. Mond, be thus recovered, but as a rule not more than 30 per cent. is practically obtained. The waste from which this fraction of the sulphur has thus been extracted is now a much more innocuous body than before, and the manufacturer is a gainer by the production of the sulphur. Still as

a fact this process is not usually worked in England, and it becomes a question whether some process of this kind may not, with advantage, be more generally adopted as a preventative against the evolution of sulphuretted hydrogen.

Another sulphur-recovery process, which is particularly applicable to the treatment of the alkaline drainage from old waste-heaps, is that proposed by Mr. Mactear. The yellow liquors thus obtained are mixed with lime and submitted to the action of sulphurous acid. Sulphur is then deposited, and calcium hyposulphite formed, whilst some sulphuretted hydrogen is given off. To this liquor, fresh yellow-liquor is added in suitable proportions, so that on the addition of hydrochloric acid no sulphuretted hydrogen is evolved, but only sulphur deposited.

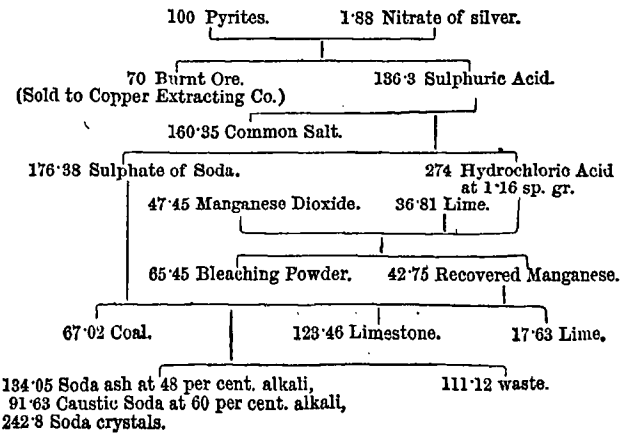
Out of the very many proposals which have been made to replace Leblanc's original reactions by others, only one has been practically successful. This is theoretically a very simple and beautiful one, and in skilful hands has been found capable of being worked on a manufacturing scale. I have here a solution of brine, such as is obtained by pumping from the Cheshire salt-beds in Northwich. This I saturate with ammonia gas, and into the brine thus saturated I pass a current of carbon-dioxide. After a short time you observe that a white precipitate falls; this white precipitate consists of bicarbonate of soda. Nothing can surely be simpler or more beautiful than this—no evolution of noxious fumes, no waste of sulphur; by a simple decomposition we at once obtain what we want; thus :—

$$NH_3 + CO_2 + NaCl + H_2O = NaHCO_3 + NH_4Cl.$$

The precipitate consists of bicarbonate of soda insoluble in the ammoniacal brine, whilst sal-ammoniac remains in the liquid, and from it the ammonia can be recovered by heating the liquor with lime. Simple and beautiful as this process appears to be, the difficulties which surround it are extremely great. Although there is doubtless a future for this process it is very unlikely that it will interfere with the manufacture of soda by the old plan. One reason against the process is, that in the old process the manufacturer not only makes use of the sodium of the common salt, but likewise of the chlorine; indeed in many alkali-works the manufac-

ture of bleaching powder is the main, that of alkali the subsidiary end. Until therefore we can find a cheap mode of extracting the chlorine from the chloride of calcium this method will hardly be much used, unless indeed Mr. Weldon's proposal for employing magnesia instead of lime and for decomposing the chloride of magnesium by steam and thus getting hydrochloric acid, turns out to be practically successful. The advantage of the process is, that the product obtained is practically pure, and the soda-ash thus prepared tests up to 58·5 per cent.

I might further enlarge upon two most important branches of the alkali trade, namely, the manufacture of bleaching powder and the manufacture of soap. The description of these is, however, rendered impossible by the limited time at my disposal, and I must conclude by referring you to some statistics of the manufacture. In the first place you have here a synoptical statement of the raw materials, products, and bye-products of an alkali works working 100 tons of pyrites from the estimates of Mr. Mactear.



Secondly you will find the following statistics of the alkali trade of the United Kingdom, lately obtained by the Alkali Association, of interest, as showing the magnitude of the interests involved in the manufacture :—

	1862.	1876.
Annual Value of Finished Products .	£2,500,000	£6,500,000
Weight of Dry Products	Tons. 280,000	Tons. 845,000
Raw Materials used : Salt	Tons. 254,600	Tons. 538,600
Coals	961,000	1,890,000
Limestone & Chalk	280,500	588,000
Lime	...	139,000
Pyrites	264,000	376,000
Nitrate of Soda .	8,300	12,200
Manganese . .	33,000	18,200
	1,801,400	3,562,000
Capital employed in the Business . .	£2,000,000	£7,000,000
Annual Cost of Materials for Repairs .	£135,500	...
,, ,, ,, and Packages	...	£700,000
Hands employed in the Manufactories .	10,600	22,000
Wages paid them annually	£549,500	£1,405,000
Weight of Alkali (Soda) exported . .	Tons. 104,762	Tons. 270,856
Value thereof	£385,245	£2,209,284

The following manufactures depend upon the products of the alkali trade :—

Soap.	Cotton.	Purification of Oils.
Candles.	Linen.	All Chemical Manufactures
Glass.	Woollen.	of any magnitude.
Paper.	Colour Making.	

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